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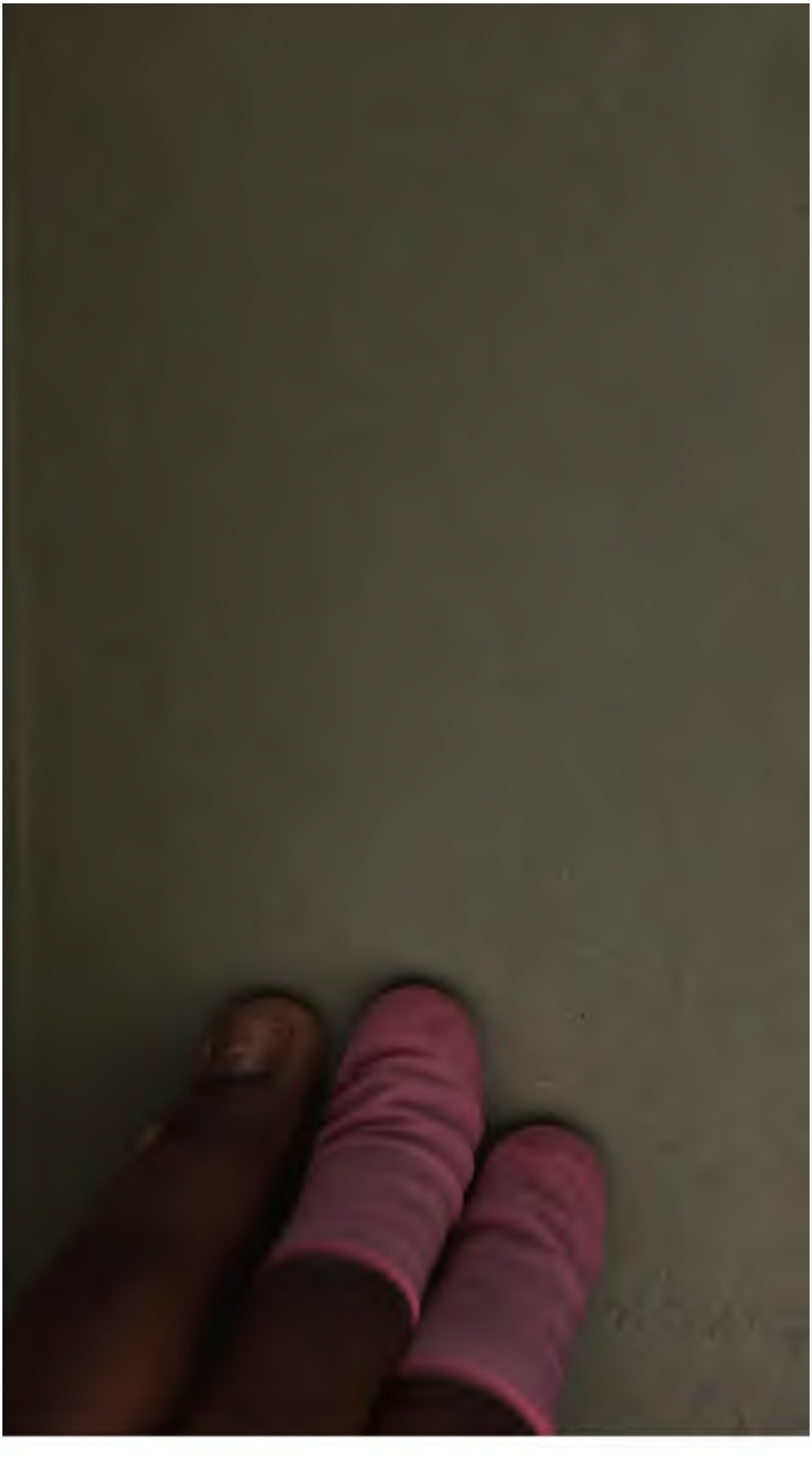

THE

EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

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Erratum in M. Studer's paper on the Geological Structure of the Alps, vol. xxxiii. p. 154, line 10 from bottom ; *for* "that we recognise it neither mineralogically nor geologically as the analogue of the macigno of the Apennines;" *read* "that we recognise it both mineralogically and geologically as the analogue of the macigno of the Apennines." *This error was in the original French memoir.*

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Fourth Letter on the Glacier Theory, to Professor Jameson.

By Professor Forbes.

GENEVA, *5th October* 1842.

My Dear Sir,—Since my last letter from Zermatt, I have had an opportunity of examining the glaciers on different sides of Monte Rosa, particularly those of Lys and Macugnaga, and those near the Valley of Saas; and on my return to Chamouni early in September, I devoted a day to each of the glaciers of Trient and Argentiére, before resuming my station at the Montanvert, where I remained until almost the last days of the month.

What I think it most interesting now to add as supplementary to my former statements, is not a description of these various glaciers, but, with particular reference to the Mer de Glace, to mention what the extended period of examination which I have been able to give to it, has enabled me to conclude beyond what is contained in my previous letters, respecting the Theory of glacier-movement generally. Having accurately observed the condition and motions of this glacier throughout by far the greater part of the season at which it, or indeed any glacier is easily accessible, or sufficiently free from snow for accurate observations,—having also, especially during the month of September, observed it under every circumstance of weather and a great range of atmospheric temperature, I believe that I have obtained the chief data necessary for basing a theory of its motion, upon sound mechanical principles. The changes which I have witnessed upon its surface, during the period of above three months during which I have studied it, are so great and remarkable, and in some respects so unexpected, as to be of capital importance in any theory which may be proposed.

I was very greatly struck with the change, in the general appearance of the glacier during my absence, from the 10th August to the 10th September. I left it comparatively high and tumid in the centre, at no great depth below the *arrête* of its natural boundary, the moraine by its side ; and fissured by crevasses, deep and rather narrow, with well-defined vertical walls.—On my return, the icy mass had most visibly sunk in its bed ; it seemed to me to have a wasted, cadaverous look ; the moraines protruded far higher than before from its sides ; and the ice itself clinging to the moraine at a considerable height above its general level, was covered by the fallen masses of stone and gravel which had rolled down the inclined plane formed by this central subsidence. The whole resembled somewhat the Wye, or some of those narrow tidal rivers whose muddy banks are left exposed by the retreat of the ocean. That this subsidence was in a good measure occasioned by the melting of the ice in contact with the bottom of the valley in which it lies, and by the falling together of the parts in a soft and yielding state, owing to a complete infiltration of the whole mass with water during the warm season of the year, was proved by a variety of circumstances which I shall not stop to detail. I may mention however, that the crevasses were wider but less deep and regular,—excessively degraded on the side to which the mid-day sun had free access, and in many places where several crevasses nearly joined, the icy partitions had sunk gradually towards a level, and thus rendered the fissured parts of the glacier more easily traversed than at an earlier part of the season. It is plain, too, that the fact of the more rapid advancement of the centre of the glacier mentioned in my earliest letter, implies a subsidence of that part, and a consequent drain from the lateral ice, to supply the vacuity which it leaves.

It will at once be understood that the change of which I speak in the external figure of the ice, its crevasses and inequalities, is an effect due to the season, and must be repeated every year. Were the summer considerably prolonged, the annihilation of the glacier would take place from a simple continuation of the process, namely, the increased velocity of the central part, the exaggeration of the crevasses in width, and the falling of their walls, or rather the gradual subsidence of the elevations, softened by the warmth, into the hollows which separate them, whilst the moraine would be left in all its continuity as a witness of the original boundary of the glacier. The ice must possess within itself some reproductive power (if the phrase may be permitted,) to restore it in spring to the level from whence it had descended ; and since crevasses thus form, extend, and again vanish,—perhaps in a single season but certainly in a very few years,—we must consider the glacier as a much more plastic body than it has commonly been imagined.

I state it, then, as a result of observation the most direct, that, in the early part of summer, the glacier level is highest, and the fissures least numerous. The latter form and widen especially during the months of June and July; and, in the beginning of August, the glacier is most difficult to traverse, (generally speaking), owing to the multitude and sharpness of these cracks ; but later, the prolonged sunshine and autumnal rains, not only reduce the ice to water, and thus carry off a part of its surface, but leave the remainder in a softened and plastic state, in which the tendency is to a general subsidence of all the elevations, whilst the prolonged excess of velocity of the central above the lateral parts, causes an increased hollowness and subsidence there, and produces a great fissuring, the lateral ice still clinging to the moraines, which it is compelled gradually to uncover. Before spring, by some process which it remains to explain, the level of the ice is restored (supposing the glacier not to be permanently wasting).

Another mode of considering the successive conditions of a certain portion of the glacier, will lead also to the admission of the ever-varying state of its aggregation and subdivision. In a glacier, like the Mer de Glace of Chamouni, which presents a great many and well-marked " accidents" of surface in its different parts, it is yet perfectly well known, that, though continually moving and changing, the distribution of these " accidents" is sensibly invariable. Every year, and year after year, the water courses follow the same lines of direction, —their streams are precipitated into the heart of the glacier by vertical funnels called " moulins ;" at the very same points, the fissures, though forming very different angles with the axis or sides of the glacier at different points of its length, opposite the same point are always similarly disposed,—the same parts of the glacier, relatively to fixed rocks, are every year passable, and the same parts are traversed by innumerable fissures. Yet the solid ice of one year is the fissured ice of the next, and the very ice which this year forms the walls of a " moulin," will next year be some hundred feet farther forward and without perforation, whilst the cascade remains immovable, or sensibly so, with reference to fixed objects around. All these facts, attested by long and invariable experience, prove that the ice of the glaciers is insensibly and continually moulding itself under the influence of external circumstances, of which the principal, be it remarked, is its own weight affecting its figure, in connection with the surfaces over which it passes, and between which it struggles onwards. It is, in this respect, absolutely comparable to the water of a river, which has here its deep pools, here its constant eddy, continually changing in substance, yet ever the same in form.

With reference to the yet more essential modifications of *structure*, I mean the veined structure which I formerly described ; I shewed in my last letter, that it is equally mutable and subjected to the momentary conditions of external restraint ; and, that far from being an original structure in the higher part of the glacier, variously modified in its subsequent course, but never annihilated, it owes its existence at any moment to the conditions of varying velocity in different parts of the transverse section of the glacier, and that it is not unfrequently entirely destroyed in one part of the glacier, to be renewed in a totally different direction in another. A molecule of ice is as passive and structureless a unit as a molecule of water, so far as it has not that structure impressed by something external at the time. Like the water in the river, myriads succeed one another, and might be mistaken for the same.

Few words will suffice to shew how intimately what I have stated is connected with the first rudiments of a theory of glacier motion, which I endeavoured to sketch in my last letter, and the truth of which all that I have since seen has tended greatly to confirm. The centre of the glacier stream is urged onwards by pressure from above (how caused we shall immediately consider), which is there resisted less than at the sides and bottom, owing to the comparative absence of friction. The lateral parts are dragged onwards by the motion of the centre, and move also, but it is quite compatible with this idea of semifluid motion, that the bottom of the glacier should remain frozen to its bed, as some writers have supposed to be the case, though I am far from asserting this to be the fact, or even supposing it probable. Why, then, are the fissures generally *vertical*, and also where a glacier is most regular, simply *transverse*, and not convex towards the lower extremity? The first of these questions had always till lately appeared to me a serious difficulty. The *fact* stated in the second, combined with the positive certainty that the centre of a glacier moves faster than its sides, in the ratio frequently of 5 to 3, shews that an answer *must* be found, and, therefore, that it offers no insurmountable objection. The explanation is to be sought in the continually varying condition of the glacier, the perpetual renewal of the crevasses, the action of water in tending to preserve verticality, and the really small variation of velocity of different parts of the ice towards the centre of a glacier of immense depth. From these circumstances, it follows that a crevasse is either renewed or altogether extirpated before its verticality is sensibly effected. For the same reason, a stick several feet long, inserted vertically in the ice, remains sensibly vertical so long as it stands at all; for the velocity of the surface is sensibly the same as that at 10 or 20, or probably even 100 feet deep in most glaciers. It is only near the bottom or bed that the velocity is materially affected, as I have found also, that, in respect to breadth, it is in the immediate neighbourhood of the sides that the velocity diminishes rapidly, and that, for half its breadth in the centre, the velocity does not vary by more than from $\frac{1}{10}$ to $\frac{1}{20}$ of its amount. It is farther worthy of notice, that whenever a glacier is of no great thickness, and, at the same time, highly inclined, that is, in circumstances calculated to produce a great difference between the motions of points of the glacier in a vertical line, there the fissures are not transverse but radiated, as in almost all glaciers of the second order, and, therefore, the fissures are not liable to distortion.

I might put it rather as a direct result of observation than as a hypothesis, that the motion of a glacier resembles that of a viscid fluid, not being uniform in all parts of the transverse section, but the motion of the parts in contact with the walls being determined mainly by the motion of the centre; but it yet remains to be shewn what is the cause of the pressure which conveys the motion, whether it is the mere weight of the semifluid mass, or the dilatation of the head of the glacier pushing onwards. The answer to this question involves the fate of the rival theories of De Saussure and De Charpentier. I still entertain the same difficulties with respect to both, which I have stated in an article in the Edinburgh Review; but these difficulties amount, I think, to a proof of insufficiency, if taken in connection with the observations which I have made this summer. On the one hand, if it were possible that the glacier could slide by the mere action of gravity in a trough inclined only 3, or 4, or 5 degrees, it is probable that one of two things would happen; either it would slide altogether with an accelerated velocity into the valley beneath, or else it would move *by fits and starts*, being stayed by obstacles until these were overcome by the melting of the ice beneath, or by the accumulated weight of snow above and behind. Now, neither of these things happen; the glacier moves on day and night, or from day to day, with a *continuous* regulated motion, which, I feel certain, could not take place were the sliding theory true.

But if possible, still stronger, as well as more multiplied, objections are to be found to the theory of dilatation, and I trust I shall not be accused of levity in thus, as it were, in a few lines, dismissing a theory which has so much *primâ facie* plausibility to recommend it, and which has been maintained with so much ingenuity by men such as Scheuchzer, De Charpentier, and Agassiz. It is essential to the aim of this letter, that I state briefly the grounds of the conclusions at which I have arrived, whilst it is equally essential that my observations should be confined within small compass. In another place I shall give them all the development that may be requisite.

Summarily, then (1.) The motion of the glacier, in its several parts, does not appear to follow the law which the dilatation theory would require. It has been shewn (Ed. Rev., April 1842, p. 77.) that the motion ought to vanish near the origin of the glacier, and increase continually towards its lower extremity. I have found the motion of the higher part of the Mer de Glace to differ sometimes very little from that several leagues farther down; whilst in the middle, owing to the expansion of the glacier in breadth, its march was slower than in either of the other parts. (2.) Whilst I admit that the glacier is, during summer, infiltrated with water in all or most of its thickness (a point on which I had last year great doubts), I feel quite confident that, during some months of the year during which the glacier is in most rapid motion, no congelation takes place in the mass of the ice beyond a depth of a very few inches, much less during the cold of each night, and least of all, at *all* times, as appears to be now the opinion held upon the subject. Whilst I say that I am confident of this, I will state one proof. Less than ten days since I traversed the Mer de Glace up to the higher part of the Glacier de Lechaud, whilst it was covered with snow to a depth of six inches at Montanvert, and three times as much in the higher part. It was snowing at the time, and for a week the glacier had been in the same state nearly, the thermometer having fallen in the mean while to 20° Fahr. Yet I had abundant evidence that the effect of the frost had not penetrated farther into the ice than it might be expected to have done into the earth under the same circumstances. All the superficial rills were indeed frozen over; there were no cascades in the "moulins;" all was as still as it could be in mid-winter; yet even on the Glacier de Lechaud, my wooden poles, sunk to a depth of less than a foot in the ice, were quite wet, literally standing in water, and consequently unfrozen to the walls; and in the hollows beneath the stones of the moraines, by breaking the crust of ice, pools of unfrozen water might be found almost on the surface. Is it possible, then, that the mere passing chill of a summer night, or the mere cold of the ice itself at all times, can produce the congelation which has been so much insisted on?

But (3), What was the effect of the congelation, trifling as it was, upon the motion of the glacier? So sharp and sudden a cold succeeding summer weather, must inevitably, it seems to me, were this theory true, have produced an instantaneous acceleration of the mean motion of the glacier. But the contrary was the fact; the diurnal motion fell rather short of its previous value, and so soon as the severe weather was past, and the little congelation which had taken place thawed, and the snow reduced to water, than the glacier, saturated in all its pores, resumed its march nearly as in the height of summer.

(4.) It has been inferred from the dilatation theory that whilst the surface of the glacier continually wastes, it at the same time heaved bodily upwards from beneath, so that its absolute level is unchanged. My experiments, as well as the most ordinary observation (as has been already remarked) disprove this hypothesis. I find that between the 26th June and the 16th September, the surface of the ice near the side of the Mer de Glace had lowered absolutely TWENTY-FIVE *feet* 1.5 *inches*, and the centre had undoubtedly fallen more. The observation of the waste of the surface by the protrusion of a stick sunk to a determinate depth in a hole, is very inaccurate, and gives results *below* the truth.

I am perfectly ready to admit, with M. de Charpentier, that the congelation of the infiltrated water of glaciers is an important part of their functions; only, I conceive that it occurs but once a year to any effective extent, instead of daily or continually, as he supposes. Every thing which I have seen on the glacier, during cold weather and when covered with snow, confirms the idea I have always entertained, that the progress of congelation in the mass of the glacier is very similar to that of a mass of moist earth, and that, therefore, the daily variations of temperature can make no sensible impression, with respect to the mass of the infiltrated ice. The prolonged cold of winter must, however, produce a very sensible effect; and considering that the temperature of the mass is never above 32°, it may be expected that the congelation of the water in capillary fissures in ice will, in the course of months of tranquillity, reach a great depth. I apprehend that there is only an annual congelation, and that its effect is not to move the glacier onwards by sliding down its bed—for that the friction of so enormous a body seems evidently to render impossible—but (what Mr Hopkins has very well shewn is the only alternative, and which he has used as an argument against Charpentier's theory) to dilate the ice in the direction of *least resistance*, that is, vertically, and consequently to increase its thickness. The tendency of such a force would, therefore, be to restore during the winter the thickness of ice lost during the summer; and in those winters which are less severe, a less depth of ice being frozen, a less expansion would occur, and a permanent diminution of the glacier would result. Nothing can be more certain than the fact, so well stated by Charpentier in his 10th section, that the glacier does not owe its increase to the snow of avalanches, nor indeed to any snow which falls on the greater part of its surface.

In conclusion, the admission of semifluid motion produced by the weight of the ice itself, appears to explain the chief facts of glacier-movement, viz. (1.) That it is more rapid at the centre than at the sides; (2.) For the most part, most rapid near the lower extremity of glaciers, but varying rather with the transverse section than the length; (3.) That it is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain, and less rapid in sudden frosts; (4.) It is farther in conformity with what we know of the plasticity of semisolids generally, especially near their point of fusion. Many examples will occur to every one of what they have observed of the plasticity of hard bodies,—such as sealing-wax, for example,—exposed for a long time to a temperature far below their melting heat, and which have moulded themselves to the form of the surfaces on which they rest. (5.) When the ice is very highly fissured, it yields sensibly to the pressure of the hand, having a slight determinate play, like some kinds of limestone, well known for this quality of flexibility. (6.) I have formerly endeavoured to shew how such a condition of semirigidity, combined with the determined movements of the glacier, accounts for the remarkable veined structure which pervades it. I am, my Dear Sir, yours very truly,

James D. Forbes.

Professor Jameson.

On the Salt Steppe south of Orenburg, and on a remarkable Freezing Cavern. By Roderick Impey Murchison, Esq. Pres. G. S.*

I. This salt steppe is distinguished from many of those which are interposed between the Ouralsk and the Volga, or are situated on the Siberian side of the Ural Mountains, by consisting not of an uniform flat resembling the bed of a dried up sea, but of wide undulations and distantly separated low ridges; nevertheless it is, Mr Murchison states, a true steppe, being devoid of trees and little irrigated by streams. The surface consists of gypseous marls and sands, considered by the author to be of the age of the Zechstein,† and it is pierced in the neighbourhood of the imperial establishment of Illetzkaya Zatchita by small pyramids of rock salt. These protruding masses attracted the attention of the Kirghiss long before the country was colonized by the Russians; but it is only during a short period that the great subjacent bed has been extensively worked. The principal quarries, exposed to open day, are situated immediately south of the establishment, and have a length of 300 paces, with a breadth of 200, and a depth of 40 feet. The mass of salt thus exposed is of great purity, the only extraneous ingredient being gypsum, distantly distributed in minute filaments. At first sight the salt seems to be horizontally stratified, but this apparent structure, Mr Murchison states, is owing to the mineral being extracted in large parallelopipedal blocks 12 feet long, 3 feet deep, and 3 wide. On the side where the quarry was first worked, the cuttings presented, in consequence of the action of the weather, a vertical face as smooth as glass, but at its base there was a black cavern formed by the water which accumulates at certain periods of the year, and from its roof were saline stalactites. The entire range of this bed of salt is not known; but the mass has been ascertained to extend two versts in one direction, and Mr Murchison is of opinion that it constitutes the subsoil of a very large area; its entire thickness also does not appear to have been determined, but it is stated to exceed 100 feet. The upper surface of the deposit is very irregular, penetrating, in some places, as already mentioned, the overlying sands and marls.

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* From the Proceedings of the Geological Society, vol. iii. part 2, p. 695; having been read March 9. 1842.

† His extensive surveys of Russia have convinced Mr Murchison, that rock-salt and salt springs occur in all the lower sedimentary rocks of that empire, from great depths below the Devonian, or old red sandstone system to the Zechstein and the overlying marls and sandstones.

In consequence of the salt occurring at so small a depth, every pool supplied with springs from below is affected by it;* and one of them used by the inhabitants as a bath, is so highly charged with saline contents, that there is a difficulty in keeping the body submerged, and the skin, on leaving the pool, is encrusted with salt. This brine swarms with animalcules.

II. Mr Murchison then describes the freezing cavern and the phenomena exhibited by it. The cave is situated at the southern base of a hillock of gypsum at the eastern end of the village connected with the imperial establishment; and it is one of a series of apparently, for the greater part, natural hollows, used by the peasantry for cellars or stores. The cave in question is, however, the only one which possesses the singular property of being partially filled with ice in summer, and of being destitute of it in winter. "Standing on the heated ground and under a broiling sun, I shall never forget," says the author, "my astonishment when the woman to whom the cavern belonged unlocked a frail door, and a volume of air so piercingly keen struck the legs and feet, that we were glad to rush into a cold bath in front of us to equalize the effect." Three or four feet within the door, and on a level with the village street, beer and quash were half frozen. A little further, the narrow chasm opened into a vault fifteen feet high, ten paces long, and from seven to eight wide, which seemed to send off irregular fissures into the body of the hillock. The whole of the roof and sides were hung with solid undripping icicles, and the floor was covered with hard snow, ice, or frozen earth. During the winter all these phenomena disappear, and when the external air is very cold, and all the country is frozen up, the temperature of the cave is such, that the Russians state they could sleep in it without their sheep-skins.

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* The abundance of these brine springs in various parts of Russia must lead, the author says, to the abandonment of Pallas's hypothesis, that the saline pools and lakes are the residue of former Caspians; though he admits, that some of the vast low steppes of the south formed the bottom of a former condition of the existing Caspian.

In order to lay before the Society an explanation of these curious opposite conditions of the cave, the author communicated with Sir John Herschel, and received the documents which follow this abstract. With respect to the observations in Sir J. Herschel's letter, Mr Murchison says, he does not conceive that the ice caverns at Teneriffe, in Auvergne and elsewhere, are analogous cases with that at Illetzkaya Zatchita, the frozen materials in the last not arising from the preservation of the snow or ice of the preceding winter, but from the peculiar condition of the cavern during the hottest summer months. He states also, that he particularly urged the authorities at Orenburg, as well as the directors of the Salines, to keep accurate registers of the temperature throughout the year, and to ascertain precisely the changes which the cave undergoes between the extremes of summer and winter. There is, he observes, a very marked difference between the climate of the steppes south of Orenburg and that of Ekaterinburg, not merely due to the difference of six degrees of latitude, but arising also from the altitude of the position of Ekaterinburg, and the shortness of its varying summers, as well as from the long droughty summers of the steppes, which are removed from all mountain chains, and possess comparatively no great altitude above the sea. In the southern region, he conceives, a substratum of frozen matter cannot exist, there being a most extraordinary difference between the climate of Yakatsk (lat. 62½° N. long 131° E.), and that of Orenburg (lat. 51° 46′ N.), the winter of the former lasting eight or nine months, with the thermometer during long periods constantly 30°, and sometimes 40° of Reaumur below zero.*

Respecting the explanation that the difference of temperature in the cave is due to the propagation through the gypsum hillock of the heat or cold of the preceding summer or winter season, Mr Murchison conceives that the fissures which ramify from the cave into the hill, present difficulties to such a solution. When he was on the spot, the existence of these fissures led him to speculate upon the possibility of the phænomena being due to currents of air passing over subterranean floors of moistened rock-salt, and on the effects which would be produced when such currents came in contact with a stream of dry heated air.

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* Mr Murchison ascertained, during his journey in the North of Russia in 1840, that much remains to be done relative to the circumstances of the recorded frozen substratum of Yakatsk; and he states the following as points requiring attention. 1*st*, With the exception of about sixty feet of alluvial soil, the whole shaft to a depth of 350 feet, was sunk through solid strata of limestone two to six feet thick, and shale with a little coal; 2*dly*, That none of the sinkings took place in summer, although renewed for several years, on account of the foul air generated in the shaft; 3*dly*, That when Admiral Wrangel descended the shaft during summer, and the surface was burnt up, he found the thermometer to stand at 6° Reaum. below zero.

Extracts from a Letter addressed by Sir J. Herschel, Bart., F.G.S., to Mr Murchison, explanatory of the Phenomena of the Freezing Cave of Illetzkaya Zatchita.[*]

That the cold in ice caves (several of which are alluded to in a part of this letter not published) does NOT *arise from evaporation*, is, I think, too obvious to need insisting on. It is equally impossible that it can arise from condensation of vapour, which produces heat, not cold. When the cold (by contrast with the external air, *i. e.* the difference of temperature) is greatest, the reverse process is going on. Caves in moderately free communication with the air are dry and (to the feelings) warm in winter, wet or damp and cold in summer. And from the general course of this law I do not consider even your Orenburg caves exempt, since however apparently arid the external air at 120° Fahr.! may be, the moisture in it may yet be in excess and tending to deposition, when the same air is cooled down to many degrees beneath the freezing point.

The data wanting in the case of your Orenburg cave are *the mean temperature of every month in the year of the air*, and of thermometers buried, say a foot deep, on two or three points of the surface of the hill, which, if I understand you right, is of gypsum and of small elevation. I do not remember the winter temperature of Orenburg, but for Catherinenbourg (only 5° north of Orenburg), the temperatures are given in Kuppfer's reports of the returns from the Russian magnetic observatories. If any thing similar obtains at Orenburg, I see no difficulty in explaining your phenomenon. Rejecting diurnal fluctuations, and confining ourselves to a single summer wave of heat propagated downwards alternately with a single winter wave of cold, every point at the interior of an insulated hill, rising above the level plain, will be invaded by these waves in succession (converging towards the centre in the form of shells similar to the external surface), at times which will deviate further from midwinter and midsummer the deeper the point is in the interior, so that, at *certain depths* in the interior, the cold wave will arrive at midsummer, and the heat wave in midwinter. A cave (if not very wide mouthed and very *airy*) penetrating to such a point, will have its temperature determined by that of the solid rock which forms its walls, and will of course be so alternately heated and cooled. As the south side of the hill is *sunned*, and the north *not*, the summer wave will be more intense on that side, and the winter less so; and thus, though the *form* of the wave will still generally correspond with that of the hill, their *intensity* will vary at different points of each wave-surface. The analogy of *waves* is not strictly that of the progress of heat in solids, but nearly enough so for my present purpose.

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[*] From the Proceedings of the Geological Society, vol. iii. part 2; having been read March 9. 1842.

The mean temperature for the three winter months, December, January, February, and the three summer months, June, July, August, for the years 1836, 7, 8, and the mean of the year, are for Catherinenbourg as follows:—

	Winter.	Summer.	Annual Mean.
1836	− 10°.93 R.	+ 11°.90 R.	+ 1°.22 R.
1837	− 12°.90	+ 12°.93	+ 0°.30
1838	− 12°.37	+ 12°.37	+ 0°.60
Mean	− 12°.07 R.	+ 12°.40 R.	+ 0°.70 R.
	+ 4°.83 Fahr.	+ 59°. 9 Fahr.	+ 33°.57 Fahr.

The means of the intermediate months are almost exactly that of the whole year, and the temperature during the three winter, as well as the three summer months, most remarkably uniform.

This is precisely that distribution of temperature over time, which ought, under such circumstances, to give rise to well-defined and intense waves of heat and cold; and I have little doubt, therefore, that this is the true explanation of your phenomenon.

I should observe that, in the recorded observations of the Catherinenbourg Observatory, the temperatures are observed two-hourly, from 8 A.M. to 10 P.M., and not at night. The mean monthly temperatures are thence concluded by a formula which I am not very well satisfied with; but the error, if any, so introduced, must be far too trifling to affect this argument. The works whence the above data are obtained are—*Observations Météorologiques et Magnétiques faites dans l'intérieur de l'Empire de Russie*, and *Annuaire Magnétique et Météorologique du corps des Ingénieurs des Mines de Russie*,—works which we owe to the munificence of the Russian government, and which it is satisfactory to find thus early affording proofs of utility to science, in explaining what certainly might be regarded as a somewhat puzzling phenomenon, as it is one highly worthy of being further studied, and being made the subject of exact thermometric researches on the spot, and wherever else anything similar occurs.

Sir John Herschel then states, that since he began this letter he had examined some old documents, and found the paper which accompanied his letter. "The date of this manuscript," he adds, "as nearly as I can collect it from collateral circumstances, must have been somewhere about the year 1829, or rather before than after. I remain, &c.

J. F. W. HERSCHEL.

P.S.—Thermometric observations in the Steppes, of the mean monthly temperature of the soil at different depths, from 1 to 100 feet (at Forbes' intervals), would be most interesting. At Catherinenbourg, the mean temperature of the air being 33°. 6 Fahr., no *permanently frozen soil* would probably be reached, but a very little more to the northward that phenomenon must occur.

The "thinning out" of the frozen stratum would be most interesting to trace, but in thinning out by decrease of latitude, it might possibly at the same time "dip" beyond reach, all above it being occupied by soil subject to the law of periodic frost and thaw, and giving room, under favourable circumstances, to ice caverns, pits, or galleries. What determines the distinct definition of the *hot* and cold alternating layers, is the exceedingly peculiar form of the curve of the monthly temperatures, as given in the tables above referred to.

On some Phenomena observed on Glaciers, and on the internal Temperature of large masses of Ice or Snow, with some Remarks on the natural Ice-caves which occur below the limit of perpetual snow. By Sir JOHN HERSCHEL, Bart, F.G.S. &c.*

In a visit to the glacier of Chamouni in the summer of 1821, I was struck with the very remarkable positions of several large blocks of granite resting on the glacier in various parts. They were perched on stools of ice of less diameter than the blocks themselves, which overhang their supports on all sides, as a mushroom does its stalk. The position of these large masses was rendered the more striking when contrasted with that of small fragments of stone, equally (to appearance) exposed to all the local heating and cooling influences, but which were uniformly found to have sunk into the ice, and that the deeper (within certain limits) the less their size. On consideration, the cause became apparent, and, as it affords a very pretty illustration of the laws of the propagation of heat through bad conductors, and the steps by which an average temperature is attained in large masses from a varying source, I will here state it as it occurred to me at the time.

With regard to the sinking of small masses into the ice when heated by the sun, it is the natural effect of the greater power of absorbing heat which stone possesses beyond ice. Whenever the sun shines, the stone will detain more of its heat than an equal surface of ice would do; and as it gives this out to the ice below *nearly as fast as it receives it*, a greater depth of ice is melted in a given time beneath the stone than in the parts around. On the other hand at night, ice radiates terrestrial heat nearly or quite as copiously as stone, and thus they are on a par in frigorific power.

The elevation of great masses above the general level, which at first sight would appear to contradict this explanation, is however equally a consequence of the laws of the propagation of heat. To conceive this, let us imagine a very large block of stone at the commencement of the summer, to lie on a level surface of ice, in a situation exposed to the direct rays of the sun, where the *mean temperature of day and night* is (even in summer) but little above the freezing point, but where, however, no fresh snow falls during the whole summer. In the day-time then, while receiving the sun's rays, the upper surface of the stone will be strongly heated, and a *wave of heat* will be propagated slowly downwards through the stone towards the ice, diminishing in intensity rapidly, however, as it travels, since each superior stratum only divides its excess of temperature with that below. Long before this can reach the ice, however, night comes on. The surface cools below the mean or even below the actual temperature of the air by radiation, and a *wave of cold* is propagated (or which comes to the same thing, heat is *abstracted* from stratum to stratum) by the same laws. This follows close on the wave of heat below, and travels with equal velocity. In consequence, the heated stratum parts with its heat, now both upwards and downwards, and thus the intensity of the wave of heat diminishes with much greater rapidity as it proceeds downwards. It is manifest, that were the thickness of the stone infinite, the wave of heat being *always* followed *close* up by the wave of cold, and a perpetual tendency to an equilibrium of temperature going on between them, they would ultimately reduce each other to their mean quantity, and (not to take the extreme case of infinity) at some very moderate depth, the fluctuations above and below the mean temperature of the air, as the successive nocturnal and diurnal waves pass through a particle of the stone there situated, will be rendered very trifling, and may for our present purpose be regarded as evanescent. Beyond this depth, whatever mass of stone may exist, may be regarded as a slow conducting mass, interposed between a surface of ice constantly maintained at 32°, and a surface of stone constantly maintained at the mean temperature of the air, which by hypothesis is very little above it. Through this, then, the heat will percolate uniformly but feebly, and the ice below will be very slowly melted, and the more so in proportion to the thickness of the interposed stratum. Let us now consider what happens to the ice on the parts undefended by the stone. In the day time these experience the direct radiation of the sun, and therefore melt and run off in water. At night, it is true, the remaining surface cools by radiation ; but this cold is propagated downwards, and on the return of day the *superficial lamina* is necessarily put in equilibrium with the air and melted by the sun, and however cold the interior of the mass may be, the surface will still be kept all day in a state of fusion. Thus the degradation of the general surface of the ice will be in proportion to the direct intensity of the sun's rays and the time they shine ; while that of the surface beneath the stone will only be in proportion to the excess of the mean temperature of day and night above 32°, diminished by the effect of the thickness of the stone. This, of course, will produce a difference of level, and a *relative* elevation of the stone sunk as really observed. One curious, and at first sight, paradoxical consequence seems to follow from this reasoning, viz., that the ice of a glacier, or other great accumulation of the kind, may, at some depth beneath the surface, have a permanent temperature very much below freezing, though in a situation whose mean *annual* temperature is sensibly above that point. In fact (continually to use the metaphorical expression already employed), there is no reason why *waves of cold*, of any intensity below 32°, may not be propagated downwards into the interior of the ice ; but waves of heat above that point, of course, never can. Thus, the cold of winter and the frost produced by radiation in the clear nights of summer, will enter the mass and lower its internal temperature ; while the heat of the summer air, and that imparted by solar radiation, will mainly be employed in melting the surface, and will run off with the water produced.

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* From the Proceedings of the Geological Society, vol. iii. part 2; having been read March 9. 1842.

I am not aware of any observations on the internal temperature of glaciers ; they are of course difficult from their usual rifty state ; but the point may not be unworthy the attention of the scientific traveller. May not this be the cause of those natural formations of ice which have been observed in caverns in Teneriffe, and on some elevated points of the Jura chain, below the level of perpetual snow ? It is obviously no matter whether the interior mass in the above reasoning be ice or rock. It is enough that its surface, during the whole or greater part of the year, should be covered with ice, to bring down the mean annual temperature of its interior materially below the temperature due to its elevation, and which it *would* have were it not so covered. Conceive, now, a mountain whose summit is in this predicament, viz. constantly maintained at a *mean* temperature below that due to its elevation. This intense cold will not break off at the level of the line of perpetual snow, which is determined by the mean temperature of the atmosphere due to elevation, but will be propagated downwards in the interior of its mass. Hence, if, at a short distance below the line of perpetual snow, where the mean diurnal temperature of the exposed part, taken at a few feet or a few yards deep in the soil or rock, is a little above freezing, we drive an adit, or take advantage of a natural fissure, to obtain the internal temperature at a much greater depth from the surface; we ought to find it below 32°, and ice ought constantly to form in such cavities.

But even when the summit of a hill is not covered with ice, and when, therefore, this particular principle does not apply, it is easy to see, on the same general grounds, that something of the same kind may obtain. It is obvious, that whenever a change of temperature on the surface of a solid takes place, a *wave* of heat or cold, as the case may be, will be propagated through its substance; and if the changes be regularly periodic, the waves will be also. Moreover, it is clear that the longer the periods of the external fluctuations are supposed, the greater will be the interval of the waves, so as to make the time taken for the propagated heat to run over them precisely equal to the period of fluctuation. Now the rapidity with which successive waves of heat and cold destroy each other is inversely as the intervals, and thus the fluctuations of temperature, depending on long periods of external change, will be propagated to greater depths than those arising from shorter periods, nearly in the ratio of the lengths of the periods. Thus the depths at which the annual fluctuations of temperature cease to be sensible will be between 300 and 400 times greater than those at which the diurnal ones are neutralized. Now it may happen, from the slowness of propagation through so considerable a depth, that the winter wave of cold (consisting of many diurnal waves of alternate, greater and less intensity) may not travel down to the adit or cavern till the hottest period of the next summer, or of many summers; in short, that if at any given time the interior of the mountain were *sounded* by thermometers down its whole axis, these instruments would exhibit alternate deviations + and — from the mean temperature of the air.

Analysis of Caporcianite and Phakolite, two new Minerals of the Zeolite Family. By Thomas Anderson, M.D. Communicated by Dr Christison.*

The minerals of the zeolite family have for many years attracted the especial attention of men of science, and the class has been rapidly extended in proportion to the progress made in its study in a crystallographic as well as chemical point of view. The first characteristic difference, originally observed long since by Cronstedt, and by him considered to be the distinguishing mark of one single mineral species, which he designated Zeolite,—namely, the property of swelling out by heat previous to fusion,—has since been found to belong to a great number of other combinations. These, although materially different from each other in crystallographic form, have proved to be closely allied in chemical constitution, in so far as they consist, without exception, of a silicate of an alkali or alkaline earth in combination with a silicate of alumina and water. It is evident, then, that the relation of the silicic acid to the base, in both terms, as well as the quantity of water, is capable of considerable variation, so that the general mineralogical formula which should embrace all the members of the zeolite family would be

$$u\,r\,S^{v} + x\,A\,S^{y} + z\,A\,q.$$

Where r represents the monatomic alkaline or earthy basis, and the terms u, v, x, y, and z, are capable of varying within certain limits.

The minerals Caporcianite and Phakolite form two new members of the above general formula. Their analysis was conducted in the following manner:—

The finely pulverized mineral was dried for several days over sulphuric acid in an exsiccator, at the ordinary temperature of the atmosphere. A certain quantity of the dry powder was then weighed in a small tube retort, and heated to moderate redness for the space of half an hour. The water thus driven off was absorbed in a counterpoised tube of chloride of calcium and weighed. Another portion of the dry powder was then dissolved in hydrochloric acid, and evaporated to dryness for the separation of the silicic acid. The dry mass was then moistened with hydrochloric acid, digested for several hours, and dissolved in water, and the silicic acid filtered off. The purity of the silicic acid was then tested by solution in a boiling solution of carbonate of soda; the undissolved matter, which consisted chiefly of silicate of lime, reproduced by the strong drying necessary for the separation of the silicic acid, was then heated to redness with carbonate of soda; and alumina and lime were precipitated respectively by ammonia and oxalate ammonia. The precipitates thus obtained, weighed and subtracted from the first weight, gave that of the pure silicic acid. The solution, after the filtration of the silicic acid, was precipitated by caustic ammonia; the precipitate, after being filtered, washed, dried, and weighed, was dissolved in hydrochloric acid, and the silicic acid left undissolved was weighed; to the filtered solution potass was added in sufficient quantity to redissolve the alumina at first precipitated. By this means iron and magnesia were left undissolved, which were again precipitated from a solution in hydrochloric acid, the first by succinate, and the second by phosphate, of soda. The weights of the silicic acid, peroxide of iron, and magnesia, contained in the phosphate, being subtracted from the first weight of the ammoniacal precipitate, gave that of the pure alumina. The solution filtered from the ammoniacal precipitate was then treated with a solution of oxalate of ammonia; and the precipitate of oxalate of lime, after filtration and washing, was heated to strong redness, and treated several times in succession with a solution of carbonate of ammonia at a gentle heat as long as it continued to gain weight; and the lime was then weighed in the state of carbonate. The solution which was left after the separation of the oxalate of lime, was then evaporated to dryness in a counterpoised platinum crucible, and the ammoniacal salts driven off by a moderate heat; after which a higher temperature was given for the purpose of melting the remaining salts. These, which consisted of chloride of potassium, chloride of sodium, and magnesia, were weighed together. By solution in water the magnesia remained undissolved, and was filtered off, washed and weighed; to the solution, chloride of platinum and spirit were added, when the double chloride of platinum and potassium fell, which was collected on a weighed filter, and from which the quantity of chloride of potassium, and thence that of the potassa, were determined. By subtraction of the weights of magnesia and chloride of potassium from the first weight, that of the chloride of sodium was obtained from which the soda was reckoned.

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* Read before the Royal Society of Edinburgh on April 18. 1842, and published in part 2, vol. xv. of the Transactions.

CAPORCIANITE.

This mineral was kindly presented to me for analysis by Professor Berzelius. It was first observed by Dr Paolo Savi at Caporciani, in the valley of the Cæcino, where it occurs in a copper mine worked by two

Englishmen of the names of Hall and Sloane, and has been described by its discoverer in his *Memorie per servire allo studio della costituzione fisica della Toscana, parte* 2^{da}, § 53.

Caporcianite conducts itself before the blowpipe in a manner perfectly similar to other zeolites, in so far as its fusibility and relation to the fluxes are concerned; but it differs from them in this much, that, previous to melting, it swells out only to a very inconsiderable degree; for it melts almost at the same instant that the swelling manifests itself.

The analysis yielded the following results:—

Silicic acid, .	52.8 oxygen contained	27.43	8.
Alumina, .	21.7	10.15	10.18—3.
Peroxide of iron,	0.1	0.03	
Lime, .	11.3	3.23	3.65—1.
Magnesia, .	0.4	0.15	
Potassa, .	1.1	0.22	
Soda, .	0.2	0.05	
Water, .	13.1	11.64	3.
	100.7		

If we here express by r the monatomic bases, then the quantities of oxygen in r, A, S, and Aq are to each other as 1 : 3 : 8 : 3, which evidently determine the mineralogical formula to be $r\,S^2 + 3\,A\,S^2 + 3\,A\,q$. This, when transformed to the chemical formula, becomes $\dot{r}^3\,\dddot{Si}^2 + 3\,\dddot{Al}\,\dddot{Si}^2 + 9\,\dot{H}$.

It thus appears that Caporcianite stands chemically in near relation with the minerals, Analcime, Ledererite, Potash-Harmotome, Chabasie, and Levyne, from which it is separated merely by the difference in the quantity of water which it contains. All these minerals consist of a bisilicate of the first as well as of the second term; and the quantity of oxygen in the alumina is in all of them three times that contained in the monatomic basis. The formulæ of these minerals are as follows:—

Analcime, .	$r\,S^2 + 3\,A\,S^2 + 2\,A\,q$	$r =$ N.
Ledererite, .		$r =$ C.N.
Caporcianite, .	$r\,S^2 + 3\,A\,S^2 + 3\,A\,q$	$r =$ C.
Potash-Harmotome,	$r\,S^2 + 3\,A\,S^2 + 5\,A\,q$	$r =$ K.C.
Chabasie, .	$r\,S^2 + 3\,A\,S^2 + 6\,A\,q$	$r =$ C.N.
Levyne, .		$r =$ C.K.N.

The formula $r\,S^2 + 3\,A\,S_2$ is thus, then, known to exist in no less than four different combinations with water, namely, with 2, 3, 5, and 6 atoms, the second of which results from the foregoing analysis.

PHAKOLITE.

This mineral occurs in small crystals in the Bohemian Mittelgebirge, and was from crystallographic investigation believed to be nearly related to Chabasie. But the following analysis shews that this supposition is not confirmed by its chemical constitution.

Phakolite, which, in its relations before the blowpipe, agrees in all respects with the other zeolites, was analyzed after the foregoing method, with this exception, that the quantity of water was determined simply by the loss of weight sustained at a red heat. The composition was found to be as follows:—

		oxygen contained	
Silicic acid,	45.628	23.708.	
Alumina,	19.480	9.097	9.221.
Peroxide of iron,	0.431	0.144	
Lime,	13.304	3.737	4.442.
Magnesia,	0.143	0.053	
Potassa,	1.314	0.222	
Soda,	1.684	0.430	
Water,	17.976	15.982.	
	99.960		

This constitution has little resemblance to that of chabasie; for the quantities of oxygen in *r*, A S and A *q*, are to each other in chabasie, whose mineralogical formula is $r\,S^2 + 3\,A\,S^2 + 6\,A\,q$, as 1 : 3 : 8 : 6, whereas those quantities in phakolite are in the relation of 1 : 2 : 5 : 3½. If we assume that the quantity of water has come out too high, which is generally the case when it is determined by the simple loss of weight at a red heat, then the constitution of phakolite would be represented by the mineralogical formula $r\,S^3 + 2\,A\,S + 3\,A\,q$, which transformed to the chemical, is $3\,\dot{r}\,\dddot{Si} + 2\,\dddot{Al}\,\dddot{Si} + 9\,\dot{H}$.

It appears, then, that phakolite belongs to that class of minerals which in the first term contain a tersilicate, and in the second, a simple silicate of the base, along with water. The minerals belonging to this class at present made out are:—

Gigantolite,	$r\,S^3 + A\,S + A\,q$	$r = fe, mg,$ K.N.
Harringtonite,	$r\,S^3 + A\,S + 2\,A\,q$	$r =$ C.N.
Mesotype,		$r =$ N.C.
Lehuntite,	$r\,S^3 + A\,S + 3\,A\,q$	$r =$ (N.)C.
Phakolite,	$r\,S^3 + 2\,A\,S + 3\,A\,q$	$r =$ (C.)K.N.
Mezolite,	$r\,S^3 + 3\,A\,S + 3\,A\,q$	$r =$ N + 2 C.
Scolezite,		$r =$ C.
Pyrargillite,	$r\,S^3 + 3\,A\,S + 4\,A\,q$	$r = fe, mg,$ K.N.
Antrimolite,	$r\,S^3 + 5\,A\,S + 5\,A\,q$	$r =$ C.(K.)

From this table it will be seen that phakolite forms a middle term between lehuntite and mezolite, and differs from them only in the second or alumina term, which in the three minerals stand to each other in the ratio of 1, 2, and 3, while the quantities of silicate of the monatomic bases and water are the same in all three.

M. Doyère's Experiments on the Revivification of animals of the types Tardigrada and Rotifera.

Shortly after the existence of swarms of animalculæ in water containing organic matters had been revealed by the microscope, the use of that instrument led to the discovery of another fact, equally unexpected, and more difficult of comprehension, inasmuch as it still more widely differed from all the results heretofore arrived at from the study of animated beings. In fact, by the examination of dry dust collected from a gutter, Leuwenhoeck ascertained the existence of an animal which, under the influence of desiccation, ceased to move, lost its form, and no longer gave any signs of life; and which, in this condition, appeared to differ in no respect from a dead body, as it were mummified, by being deprived of the fluids necessary for all animal existence; and yet which, after having been preserved for a long period in this dried condition, was restored to life by contact with a drop of water. Leuwenhoeck did not perceive the whole extent of the singular fact which he had thus discovered, with respect to the Rotifer of house roofs, and did not pursue his researches farther on this subject; but a phenomenon of this kind could not fail to excite lively curiosity among zoologists, and to give rise to long controversies, as well as to interesting experiments. It may be remarked that the discovery of Leuwenhoeck soon ceased to be an isolated fact in science, for Needham announced that the eels of mildewed corn possessed, like the Rotifera, the faculty of reviving after having been completely dried; and Spallanzani arrived at the same result, after observation, not only of the Rotifera and Anguillula, but also of another microscopic animalcule, to which he gave the name of Tardigrade (R. tardus).

The investigations of this skilful observer were numerous, and conducted with the profoundly scientific spirit which characterizes all his labours, and might perhaps have been deemed sufficient to convince naturalists as to the truth of the fact, and to serve as a basis to subsequent inquiries.

But the results thus obtained carried little weight, and it would be easy to give a long list of naturalists, who even at present deny, in the most positive manner, what has been termed the *Revivification of Rotifera.*

Latterly, it is true M. Schultz has successfully repeated some of Spallanzani's experiments, and has furnished many naturalists with the opportunity of making similar researches; but still more lately, M. Ehrenberg has added the weight of his great authority to the opposite opinion; and having formally rejected the opinion of Spallanzani, has attempted to explain the way in which an error of the kind could find its way into science.

This interesting and much debated question, then, could not be considered as definitely settled, and appeared to demand further investigation. It was necessary to examine carefully all the circumstances attending the phenomena described by Leuwenhoeck, Needham, and Spallanzani, to submit to the proof of experiment, the objections and hypotheses presented by others, antagonists of these celebrated observers, and to acquire new facts by which one or other of the contradictory opinions of naturalists might be supported or refuted. This difficult task has been undertaken by M. Doyère.

The *Rotifera* and the *Tardigrada* are found, as is well known, in the moss growing upon roofs, or in the sand found in the gutters of the roof, and are seen in the living state when these matters, after having been for a long time dry, are wetted with water. The fact of the appearance of these animalculæ in a living state in dust which had been dry during months, or even whole years, can no longer be disputed, and it is equally well demonstrated that, with these minute beings as with animals of a higher class, evaporation of their fluids, carried to a certain extent, induces the abolition of every sign of vital motion. The partizans of Spallanzani's opinion regard the reappearance of these living beings as a sort of resurrection; and the advocates of the contrary opinion think that the phenomena may be explained in a simpler manner; the opinion is, that the Rotifera, &c. are of an amphibious nature, and capable of living in dry air as well as in water or sand, where the moss with which they are surrounded would preserve them from too complete desiccation, so that in fact, in the above cited instances, the active state of the animalculæ would never even be interrupted, and these little animals buried in apparently dry dust, would still meet with sufficient humidity to prolong their lives and to allow of reproduction, so that those which have been supposed to become revivified would be in reality, to use the expression of Ehrenberg, only the great grand-children of those observed in the same material at the commencement of the experiment. According to other naturalists, the desiccation of the sand or moss containing the Rotifera, would infallibly kill the animals themselves, but would not destroy the vital principle in the ova which they may have deposited, and consequently, instead of witnessing the resurrection of the animals themselves, we only see the ova rapidly developed by the influence of the water, and giving birth to animalculæ whose growth would be equally rapid.

Finally, there are other physiologists who consider that the Rotifera, &c., of dry sand, do not undergo a complete desiccation, but such a degree of it only, as to plunge them into a sort of torpor, and conceive that these animalculæ, although to all appearance dead, yet preserve a latent life, but still a real life sufficient to establish a bond of connection between the active life which precedes the evaporation of the fluids, and that equally active, when they are restored by the addition of humidity, to the full exercise of their functions. The observations of M. Doyère overturn all these hypotheses, and confirm, in the clearest way, the results obtained by Spallanzani.

Thus, in answer to the arguments employed by Ehrenberg, it is sufficient to observe, that living Tardigrada are never found in the dry dust of gutters ; but that, by the aid of the microscope, corpuscles can be seen which entirely resemble the dead bodies of these animalculæ, deformed by desiccation ; and that in matters where no living being was previously discernible, living Tardigrada frequently appear on the addition of a little distilled water. M. Doyère is even assured that it is not impossible to revivify these animalculæ, if taken one by one, and dried separately on pieces of glass, without being surrounded by sand or other material, organic or inorganic, capable of preserving them from the ordinary effects of evaporation. In his experiments, he has been able to count them, and to trace in each separate individual all the phases of desiccation; to observe them gradually assume the appearance of dead bodies, and to determine afterwards that these same bodies, dry and brittle, are susceptible of reassuming their primitive form, and of returning to life, under the influence merely of a few drops of water.

This experiment appears to be decisive; but it may still be asked, whether the drying which the animalculæ have undergone has been complete, and if the privation of all the water contained in their tissue, would not render them incapable of resurrection, after having in this way passed years in a state of apparent death?

In order to determine satisfactorily this highly interesting and physiological question, M. Doyère had recourse to the most powerful means by desiccation employed by chemists in the analysis of organic substances. He suspended for five days, in the vacuum of the air-pump, over a vessel containing pure sulphuric acid, some Tardigrada surrounded with sand, or uncovered and dried upon slips of glass; and he left others during thirty days, in the Torricellian vacuum, dried by chloride of calcium; and in all these instances, he obtained some resurrections. These results are of great importance towards the solution of the question which M. Doyère had proposed to himself; but he still conceived that they might be considered as offering only a strong probability in favour of the complete desiccation of the animalculæ, in which the faculty of becoming revivified was retained; he continued his experiments, and by studying the influence of elevated temperatures upon these singular beings, he arrived at the discovery of most decisive and surprising facts.

It is well known that animals perish when their temperature is raised above a certain limit; inferior, however, to that at which the white of egg coagulates, and which in the majority of cases does not exceed 50° cent. (122° F.) Animalculæ capable of resurrection are not exempted from this law. M. Doyère is satisfied that the Rotifera and Tardigrada perish when the water in which they swim is heated to 45° cent. (113° F.), and that they cannot then be recalled to life by any means. But he has found that this is not the case when the animalculæ have been previously dried. If, instead of experimenting upon Tardigrada in full life, it is done upon individuals which have lost all their humidity by the ordinary means of desiccation, and which appear as dead, it is possible, without depriving them of the faculty of reviving, to raise their temperature to a degree which would necessarily involve the disorganization of all living tissue containing any water beyond that chemically combined with its constituent principles. In an experiment repeated in the presence of the commission of the Academy, a certain quantity of moss, containing Tardigrada, after having been properly dried, was placed in a stove, and around the bulb of a thermometer, the stem of which extended out of the apparatus; heat was gradually applied, until the thermometer thus placed in the centre of the moss indicated a temperature of 120° cent. (248° F.) This considerable heat was maintained for several minutes; nevertheless, some of the animalculæ contained in the moss returned to life, and appeared in their usual condition after they had been placed for 24 hours in a suitable degree of moisture. In other experiments, M. Doyère submitted some dried animalculæ to a heat of more than 140° cent. (284° F.), and still witnessed some of them revive after immersion in water. These facts are in themselves of considerable importance towards the solution of the question at issue, and the result, without doubt, depends upon the circumstance first pointed out by M. Chevreul, that albumen, deprived of its water by previous drying, can be submitted to a much higher temperature, without, in consequence, losing its solubility, than it could be if exposed to the same temperature in the moist state; and from the simple fact that a Tardigrade, exposed to the action of a temperature of 120° cent. (248° F.), can still be made to revive, it may be concluded, with great probability, that the whole of the water chemically free in its body had been dissipated, a degree of desiccation which would preclude all idea of vital movement. Thus the Tardigrada and Rotifera, when dry, and retaining the property of living when moistened, cannot be considered as actually alive; and their mode of existence can only be compared to that of a seed, which is organized so as to live, and which will live when exposed to the influence of air, of water, and of heat, but which, in the absence of one of these excitants, manifests no sign of activity or life, and can be preserved thus for ages, although the duration of its real life may not exceed perhaps a few weeks.

M. Doyère has also given a detailed and excellent account of the anatomy of these animalculæ, including, especially, the nervous and muscular systems; and his work is illustrated with beautiful and exact figures.*

On the Light of Lampyris Italica. *By* M. W. Peters.

The Lampyres have been the subject of a great number of researches in reference to their luminous organ; but in regard to the *Lampyris Italica,* we scarcely possess more than the observations of Carrara, according to whom this species is provided with a particular aërial sac, which, proceeding from the mouth, conducts the air to the luminous organ. This particular apparatus ought to be the cause of the differences in the luminous state, since the species of the North of Europe diffuse a continuous, equal, and tranquil light, while that of the Italian species is emitted in sparks. "It is on account of this difference," says M. Peters, "that I had a great desire to find an opportunity of examining the last-mentioned animal. This I at last obtained, during a long stay at Nice, and I did not allow it to escape, in the hope that with a good microscope I should succeed in discovering something positive, both respecting the structure of the phosphorescent part itself, and its relations with the other organs.

From the middle of May till the middle of the month of July, when walking in the vicinity of Nice after sunset, one is surprised at the curious spectacle then presented by the millions of small scintillating lights creeping about in every direction, sometimes illuminating the point of a rock—sometimes lighting a deep cavity—sometimes suddenly producing, as with a magician's wand, a brilliant illumination on the dark trunks of the olive trees,—a scene which, continually shifting and changing, is of the greatest interest. This appearance is renewed every evening; but it appears to me to be the more brilliant the greater the degree of humidity in the air. The interval between the scintillations is variable,—sometimes longer—sometimes shorter; and if one of these animals be examined while it is in a phosphorescent state, it is soon seen that the luminosity is intermittent, and that it only appears when

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* Vide Annales des Sciences Naturelles, 2d Series, 9th year, tome xiv. p. 269; tome xvii. &c. p. 193; tome xviii. p. 54. Microscopical Journal, vol. ii., No. 20, p. 251.

the animal has traversed a space of one or two feet, but that while it traverses that space, it emits a permanent light, which produces a band of very brilliant fire. When the animal is in repose, I have often counted from 80 to 100 luminous discharges in a minute; it then remains for a pretty long time without phosphorescence. There always remains a slight luminosity, which is never wholly extinguished, at the point of the body from which the luminous discharges are made. The luminous region, in the male, extends along the under side of the belly, between the fifth segment (from the anal extremity) and the penultimate one, with very nearly an equal degree of intensity; but, in the female, it scarcely occupies more than the fifth segment, and is even concentrated at its sides. If we observe this phosphorescent organ with a glass while it is emitting sparks, we notice in it a tremulous or undulatory movement, as when molecules are in motion. If we remove the luminous organs, and expose them to the air free, they shine with the same intensity as in the living animal, until their light becomes gradually extinguished. If they be rubbed against some body, the place shines for an instant with a greenish light, which can be made to reappear after becoming extinct by pouring a little water upon it. When the belly of the insect is opened, and the adjacent portions of the intestines removed without injuring the phosphoric organs, the latter continue to shine as before, but this luminosity ceases on the instant that the head is separated from the trunk.

According to these observations, are we not permitted to conclude,—1st, that it is not necessary that a globule of air should proceed from the head in order to produce these sparks, since the removal of the anterior and most essential parts of the trunk exercises no influence on the phosphorescence; 2d, Since the removal of the head immediately causes the luminosity to disappear, is this not a proof that the phenomenon depends on the will of the animal?

I believe it is quite unnecessary, continues M. Peters, to refute in this place the opinion of some observers, such as Roda and Murray, who affirm that many Coleoptera enjoy the same faculty of absorbing the solar light, and emitting it again at pleasure, since the Lampyris shines in the night even when it has been protected all the day from the solar light. Nay more, I kept some individuals in darkness for upwards of eight days, and they shone with as much intensity and splendour as before.

In order to study the *organa lucifera* more at my leisure, I carefully removed all the dorsal part of the skeleton, and exposed the intestines, which were filled with air. In the females, the ovaries immediately appear, as they fill a large portion of the interior of the body; while, in the males, we notice behind the posterior canals the deferential and seminiferous canals rolled upon themselves. Neither the bodies nor fluids contained in these canals possess luminous properties; and these two organs, very distinct from those of the phosphorescence throughout their whole extent, both open into a rectum of a very delicate structure. It was probably this delicate structure of the extremity of the intestinal canal that made Carrara suppose that it communicated with the luminous apparatus; but with the exception of the alternate dilatation of this conduit, we find no bubble of air throughout its whole extent. The phosphorescent organ is even separated from the intestines by a cushion of white fat, which can be easily raised, when we get a view of this organ, the colour of which is sulphur-yellow. On the two penultimate segments, and partially even on that which precedes them, we notice a multitude of tracheal ramifications converging, and these, when examined with the glass, appear to consist of round corpuscles closely pressed against each other, in such a way that the whole presents some resemblance to the electrical organ of the Torpedo, although I am unable to determine the degree of resemblance that may exist between the two organs. If a stronger magnifying power be used, we notice in the luminous part regular series of brownish corpuscles, having a silvery white point in the middle, which, seen with a still higher magnifying power, presents itself under the appearance of small ramifications. When a compound microscope is used, we then distinctly see that the whole organ consists of a regular bed of small spheres, into which the tracheal ramifications penetrate, and then spread themselves in the most elegant manner, forming, so to speak, the skeleton. Besides that, we see developed in this delicate membrane of small spheres a quantity of molecules, to which is attached the luminous extremity; the latter, by means of the considerable interlacement of aërial vessels, may receive an enormous quantity of air at once.

The luminous substance itself is of a yellow colour; the intensity of the light is in the direct ratio of the change of the yellow colour of the organ, which can be easily shewn when we bring the latter in contact with water. I was unable to trace the progress of the nervous system in it, because the principal branch consisted of a filet of extreme tenuity.

It must not be here supposed that we witness, in these spheres producing the phosphorescence, a transformation of the ordinary corpuscles of the fatty matter, for the former are completely different from the latter, as well in respect of form as of colour; the same in all their contours, such as they are observed by the microscope; but it appears to me likely that the principal matter entering into their structure, independently of the ramifications of the tracheæ, is a fatty matter, and that it is to the latter the luminous and phosphorescent substance is attached.

It therefore appears to me demonstrated, says M. Peters in conclusion, that the luminous organ in *Lampyris Italica*, has the most intimate relation with the organs of respiration; but I cannot determine if this is equally the case with the sexual organs."*

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* From L'Institut. No. 432, p. 127, where the paper is translated from Archiv. für Physiol., &c., 1841, p. 229.

On Coral Islands and Reefs, as described by Mr Darwin. By CHARLES MACLAREN, Esq., F.R.S.E.*

Coral islands are one of the wonders of Natural History. That masses of rock, many leagues in extent, should be founded in the depths of the ocean, and built up to the height of hundreds of feet, by minute animalculæ scarcely visible to the naked eye, is a phenomenon calculated to stagger the unlearned, and which even philosophers were slow to believe. The structure and arrangement of the mineral masses thus produced, are not less singular than their origin, and present problems which have puzzled and divided men of science. An excellent work on the latter branch of the subject has been recently published by Mr Charles Darwin, in which this able naturalist has condensed and systematized his own observations and those of his predecessors, and, for the first time, presented us with a complete view of these singular objects. The facts have led him to some new and highly curious conclusions bearing on the past and future physical history of the globe. An outline of these may not be without interest.

Corals—What they are.—The term coral includes two objects—the animal, called the Polype or Polypifer, and the tenement in which it lodges, called the Polypidom, or, more usually, the "Coral." The solid massive corals, which form reefs and islands, are chiefly found in tropical seas, and it is of these we mean to speak.

Polypes cannot live unless constantly immersed in water, or beaten by the surf: even a short exposure to the sun kills them; and hence the reefs they build terminate below the surface, sometimes one or two feet, sometimes several fathoms. Different species inhabit different depths. Some slender branching corals are found living (that is, tenanted by *living* animalculæ) at the depth of a thousand feet; but the massive corals which constitute reefs, do not exist at a greater depth than 20 or 30 fathoms; and there are species which delight in the surf, and carry on their labours amidst breakers which would swamp a boat. All the varieties included in coral reefs are not known with certainty. Those found near the top by Mr Darwin were the Porite and Millepore, and at a greater depth the Madrepore and Astrea are believed to exist. On the exterior margin of the reef at the surface, the Porites were in irregularly rounded masses from four to eight feet broad, nearly of equal thickness, and divided from each other by narrow crooked channels about six feet deep. Other parts of the reef were composed of thick vertical plates (*Millepora complanata*), intersecting each other at various angles, and "forming an exceedingly strong honeycombed mass." Between these plates and in protected crevices, a multitude of branching corals live, and the lagoon is inhabited by a distinct set of corals, generally brittle and thinly branched. The Nulliporæ, which have no visible cells, and though resembling corals, are supposed to be plants, occasionally cover the Porites and Millipores up to the level of high water.

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* This Article is slightly abridged from the original.

Coral Reefs and Atolls.—These reefs are submarine rocks of coral, usually ascending so near to the surface of the sea that their existence is indicated to the navigator by breakers. They are found remote from land, are in vast numbers, and often of great extent, and generally affect an irregularly circular form, having a pool of comparatively still water in the middle, called a *lagoon.* Storms throw up masses of broken coral upon them, which accumulate to the depth of some feet above high-water, forming chains of islets along the reef. The whole reef in this condition is called a "lagoon island," or more conveniently an "atoll," a word borrowed from the South Sea islanders. Some reefs have many islands upon them, some have few, and some have none.

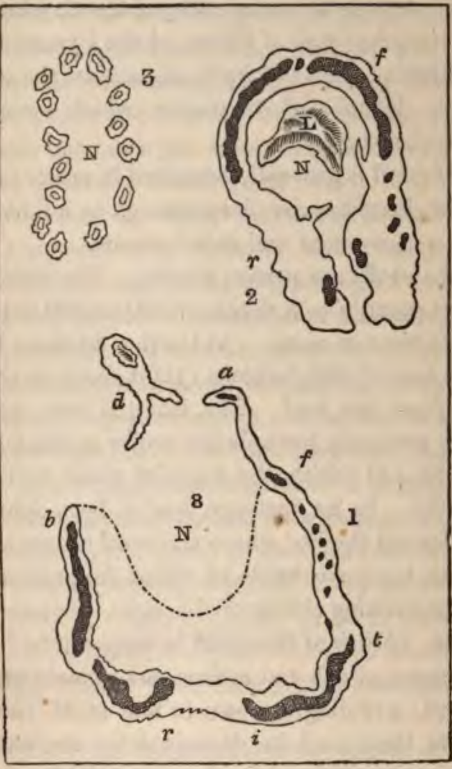
A coral reef may be defined a wall or mound of coral rock, built up in the ocean from a considerable depth, and generally returning into itself, so as to form a ring, with a sheet of still water in the interior. "Every one," says Mr Darwin, "must be struck with astonishment when he first beholds one of these vast rings of coral rock, often many leagues in diameter, here and there surmounted by a low verdant island with dazzling white shores, bathed on the outside by the foaming breakers of the ocean, and on the inside surrounding a calm expanse of water, which, from reflection, is of a bright but pale green colour." The wall of coral rock forming the ring, is generally from a furlong to half a mile in breadth, averaging about a quarter of a mile. In one rare case it is three miles. The d r of the atoll, or circle formed by the reef, varies from tha ne mile to 30 or 40. There is one 50 miles in length b 0 in adth; so that, if the ledge of coral rock forming ring were extended in one line, it would be 120 miles in th. Assuming it to be a quarter of a mile in breadth, and feet deep, here is a mound compared with which the walls Babylon, the great wall of China, or the Pyramids of Egypt, but children's toys—and built too, amidst the waves of the ean, and in defiance of its storms, which sweep away the ost lid works of man.

The all of coral is generally breached in one or more places; nd w n the breaches are deep enough to admit a ship, the toll ords a convenient and safe harbour.

S e of the atolls are perfect circles. The external side of the f often plunges to a depth of 200 or 300 fathoms, at an angle of 45 degrees or more. At Cardoo Atoll no bottom was found with a line of 200 fathoms (1200 feet), at the distance of 60 yards from the reef. The internal side, on the other hand, shelves gradually towards the centre of the lagoon, forming a saucer-shaped cavity, the depth of which varies from one fathom to fifty. In no instance has it been found entirely filled up. Beyond the line where the coral ceases to grow, the bottom of the lagoon consists of rolled fragments of it, or a whitish mud consisting chiefly of the same substance in a comminuted state. Much of this mud is supposed to be produced by certain species of fish and molluscous animals which browse upon the coral; grinding it down to fine meal, part of which will pass from them and be deposited by the water. From this description it will be seen that an atoll closely resembles in form the cone of a submarine volcano, the coral reef representing the rim, the lagoon occupying precisely the place of the crater.

The islets formed on these reefs are very singular objects. In storms, the sea throws up fragments of coral, sometimes mixed with sand. The outer and lowest stratum of this matter, which is bathed by the sea at high tide, is sometimes converted into a brecciated coral rock by calcareous infiltrations from the water. Above this, and generally at the distance of 200 or 300 yards from the outer margin of the reef, the loose fragments cast up in strong gales, mixed occasionally with sand

and shells, accumulate till they form a bank rising from six to twelve feet above high water, with the highest side towards the sea, from which the surface slopes inward to the lagoon. The ordinary width of these islets is under a quarter of a mile, and their length varies from a few yards to several miles.



In the above cut, No. 1 is a plan of Keeling Atoll, in S. latitude 12°., and E. longitude 96.54°, the structure of which Mr Darwin examined with peculiar care.

a, d, b, r, i, t, f, the coral reef; the scale being ¼ of an inch to the mile, the largest diameter of the atoll is 9 miles, and the shortest 7.

N, the lagoon, which, a little northward of the centre, is 8 fathoms deep, as marked in the figure. The part south of the dotted line is nearly dry at low water.

i, t, the dark space here on the surface of the reef, is a long narrow islet of an irregular figure. There are other two between *b* and *r*; smaller ones at *f, d,* and *a*; and others of very minute size between *f* and *t*.

There is a wide breach in the reef between *b* and *d*, and a narrower one between *d* and *a*, either of which admits a ship.

The island abounds in cocoa trees, sprung from nuts brought by the currents of the ocean from Sumatra or Java, 600 miles distant. Turtles browse on the sea-weeds which grow in the lagoon. The islands are inhabited, and these two articles supply the people with food. What is singular, fresh water is obtained from wells which ebb and flow with the tides. Mr Darwin thinks that the rain water being specifically lighter than the salt, keeps floating on its surface, and is subject to the same movements.

Barrier Reefs.—Besides the atolls, which have merely a sheet of water in the interior, there are many reefs in the Pacific and Indian Oceans which encircle one or more islands of primary, secondary, or volcanic rock. To these Mr Darwin gives the name of "barrier reefs," and the water which separates the islands from the reef is called "the lagoon channel." These reefs resemble the others in all respects. They support scattered lineal islets; they are pierced by breaches; their exterior sides are steep and deep, while their interior are shallow and slope gently. Fig. 2. represents one of these (Maurua) on the same scale as the last.

r, f, the reef, with two long narrow islets at its northern end, and some smaller ones at other parts.

N, the lagoon channel. The narrow entrance on its south side ha from four to five fathoms of water.

L, an island 2 miles long, and 800 feet high in the lagoon.

In this instance, the lagoon channel, separating the island from the reef, is of small depth and narrow, the breadth ranging from a furlong to a mile; but in other cases, it is 20 miles broad and 60 fathoms deep; and, instead of one or two islands, almost filling the lagoon (as at Raiatea), there are sometimes four, six, or more, of small size, forming mere spots in it. This is exemplified at Hogoleu and Gambier Islands. There are two very remarkable barrier reefs known. The first is that which runs along the north-east coast of Australia 1000 miles in length. It is divided from the land by a lagoon channel from 10 to 30 miles broad, and from 10 to 60 fathoms deep. The other runs parallel to the shores of New Caledonia for a length of 400 miles. It accompanies the shores for 250 miles, and continues for 150 miles more in the same direction, affording presumptive evidence that the island has a submarine prolongation of this extent. At some places it is but a few yards from the island; at others it is 20 miles; and so steep was its exterior side found to be in one instance, that at two ship-lengths from the reef no bottom was found with a line of 900 feet.

Double and triple Atolls.—There are small atolls sometimes placed in elliptical rows, with a sheet of water in the centre, and thus becoming constituent parts of a large atoll. This is shewn at fig. 3, where 14 small atolls, each with its little lagoon, are so arranged as to form one large atoll, with a large lagoon, N, in its centre. The figure is ideal, but we have an example in the Maldiva Archipelago, where the combination is carried a stage higher. This group extends over a space of 470 miles in length by 50 in breadth, and forms, as it were, three orders of atolls. First, you have a hundred of these little reefs, with pools in the centre, so disposed as to form one large atoll, 50 or 60 miles long, by 10 or 15 broad, with a lagoon 25 fathoms deep. Next, twenty of these large atolls of the second order, are arranged in the shape of a narrow ellipse, so as to form one vast atoll of the third order, 470 miles in length by 50 in breadth, with a lagoon in the interior of unfathomable depth.

The atolls and barrier reefs are dispersed in great numbers over the Pacific and Indian Oceans. *Are they the remnants of a former continent which has disappeared, or is disappearing,* from that vast watery waste ?—*or are they the harbingers of a new continent which is coming into existence ?* These are the questions which Mr Darwin has discussed with great learning and ingenuity.

Fringing Reefs.—The third form in which coral-reefs present themselves is, that of *Fringing Reefs*, the difference between which and the other two must be pointed out. "Atolls" are rings of coral-rock, rising nearly to the surface of the sea, with or without islets of drifted coral generally having a great depth of water on the outside, and a lagoon from 5 to 50 fathoms deep in the centre. "Barrier reefs" are exactly similar, except that they encircle one or more islands of sedimentary or volcanic rock, from which they are divided by a lagoon-channel, which, like the lagoons of the atolls, is generally from 5 to 50 fathoms deep. "Fringing reefs" resemble barrier reefs, except that they have a comparatively small depth of water on the outside, and small shallow lagoon channels between them and the land. They are generally found in seas that shelve gradually. The distinction between the last two classes of reefs has reference chiefly to theoretical considerations, as will be shewn by and by.

Theory of Atolls—Land that has subsided or is subsiding.—It must be kept in mind, as already stated, that reef-building corals do not live at a greater depth than 20 or 30 fathoms, or, to take the extreme in round numbers, say 200 feet. This fact is of fundamental importance in reference to every theory of coral reefs.

1. The earliest opinion was, that these reefs were built up in the ocean from unfathomable depths. But this is at once disposed of by the fact just stated.

2. At a more recent period some naturalists, struck by the generally circular form of the reefs, and the steepness of their exterior sides in many instances, supposed that they were based on the craters of submarine volcanoes. To this idea there is the conclusive objection, that it does not apply to long narrow reefs like Bow Atoll, 30 miles by 6, or Menchikoff Atoll 60 miles in length, or the larger rings, composed of smaller rings, of the Maldives. That submarine craters, if they reached the proper height, would afford fit foundations for atolls, is probable, and such may exist; but that all the numerous atolls scattered over the ocean rest on such a basis is inadmissible.

3. It has been supposed that the atolls rest on the summits of the submarine mountains. But this fails in explaining the existence of those which appear in groups. The low Archipelago, for instance, contains 80 atolls, scattered over a space of 840 geographical miles by 420, and not a single island of ordinary rock. How can we believe that a chain or group of mountains extending over such a vast area had 80 summits, all reaching within less than 200 feet of the surface, and not one rising above it? And this is not a solitary case; for the objection applies equally to the Gilbert group, 300 miles in length; the Marshall group, 520 miles by 240; and the Maldive and Lacadive group, 1000 miles in length by 100 in breadth—none of which contain a single island of any other

material than drifted coral, resting on the edge of the submarine reef. The argument holds equally good against the hypothesis of submarine craters; for so many hundreds of these could not approach within a few fathoms of the surface, without some of them rising above it.

4. Banks of sediment might (as some suppose) serve for a basis to atolls in shallow seas; but to assume the existence of hundreds of such banks of moveable matter in the profound depths of the ocean, is absurd; and it is positively disproved in the case of those atolls whose exterior sides are steeper than the cone of a volcano, descending, as some of them do, at an angle of 40 or 50 degrees.

The theory adopted, whatever it is, should also explain the existence of barrier reefs, which are analogous to atolls in every point, except that of having solid land within them. How, for instance, on any of the theories proposed, are we to account for the great barrier reef of Australia, with 60 fathoms of water even on its inner side, and descending on its outer side to unfathomable depths at a high angle? Are we to assume that there is a submarine precipice here 1000 miles in length, on which it rests.

The only hypothesis, Mr Darwin observes, *which solves all difficulties, is that which assumes that the atolls rest on land which has subsided*, and part of which was once dry. Detached atolls far from others, *may* stand on submarine rocks which have undergone no change of position; but those found in groups mark the site of land which has subsided. In short, the *atolls*, according to Mr Darwin's theory, *may be regarded as the vestiges or foot-prints of land which has disappeared; and the islands, encircled by barrier reefs, as remnants of land now partly submerged, and perhaps in progress towards final disappearance.*

As the coral animalculæ do not live at a greater depth than 200 feet, it follows that all reefs, however deep, must have begun in shallow seas; in other words, they must have been originally of the nature of "Fringing Reefs."

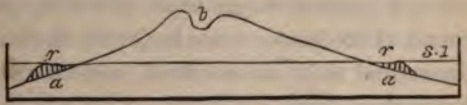
Let us suppose an island 350 feet high to exist in the tropical seas. The animalculæ commence their labours on some spot, and at a distance from the shore, as turbid water is pernicious to them. But since they cannot exist at more than

200 feet beneath the surface, they are checked in their progress seaward, and therefore continue their work to the right or left, keeping always within the requisite depth; and thus their instinct guides them to form the reef in the shape of a girdle round the island, following the sinuosities of its shores, keeping nearer them where the water deepens rapidly, and farther off where it deepens slowly. Here we have a reason why reefs may be circular, oblong, or of any other form which islands assume. Mr Darwin's plates of Raiatea and Vanikoro are good examples of the manner in which reefs adapt themselves to the outline of the islands they encircle.

The little architects carry up their fabric to the level of the low water line, and there they stop. Suppose the island now to subside 200 feet, either suddenly or slowly. They then commence a new fabric on the top of the old, and again carry it up to the low water level. But the island itself, besides losing 200 feet of height, is contracted in breadth from its low shores being covered with water; the channel between it and the reef becomes broader and deeper; and the reef having its basis at a depth beyond that where living coral exists becomes a "barrier reef."

Suppose the island to subside other 200 feet. A third fabric of coral now rises on the top of the second, till the reef again reaches the low water level. But the island itself has disappeared, and the lagoon which occupies its place, with the encircling reef, now forms an "atoll."

The subjoined figures illustrate what has been stated, and shew the process by which a "Fringing reef" passes into a "Barrier reef," and a barrier reef into an "Atoll."

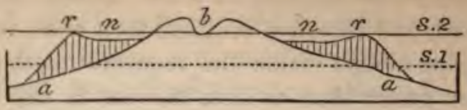


First Stage—The Fringing reef.

a b a—A section of an island, roughly copied from one given by Mr Darwin.

S 1—The surface of the sea.

r r—A fringing reef formed within a small distance of its shores.



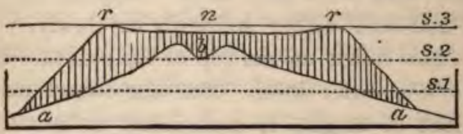
Second Stage—The Barrier reef.

a b a—The island having subsided 200 feet, is now more than half submerged; but its double summit is still visible.

S 2—The surface of the sea in its second position.

The fringing reef now raised to the level of *S* 2, forms *r r*, a "Barrier reef."

The small gutter which divided the reef from the island, is enlarged to the wider and deeper cavity *n n*, and forms a "lagoon channel."



Third Stage—The Atoll.

a b a—The island having subdivided other 200 feet, is now completely submerged.

S 3—The sea in its third position.

The barrier reef having 200 feet added to its height, now rises to *r r*.

A broad lagoon *n*, now occupies the place of the island, and the reef becomes an "Atoll."

Mr Darwin endeavoured to collect some positive evidence of subsidence in the islands, but it is not very satisfactory. Geology, however, renders it certain that some portions of the earth's surface have sunk to a lower level. The subsidence assumed, therefore, involves no inconsistency; and it enables us to account for the otherwise puzzling fact, that though corals do not live at a greater depth than 200 feet, yet numerous reefs are found 1000 feet or more in depth, the basis of which, as the steepness of their sides attest, can scarcely consist of any thing else than coral.

It explains also the appearance of the atolls in groups. Suppose a tropical island, like Ireland in size, to sink under the waves by slow stages. The hills being of different heights, the corals would begin their work on those first submerged—that is, the lowest—and new reefs would be founded successively on the higher ones as they descended, one after another, to the proper depth. When the whole island had disappeared, a group of isolated atolls, scattered over a space of 250 miles by 150, would mark the place it occupied, and indicate its figure. All the atolls would be built up to the level of low water; and while the last founded might be only two or three fathoms deep, the first might be two or three hundred. In this way, the lower hills might have their representative reefs as well as the higher, though the creatures that construct them can work only at limited depths.

Again, if the principle be correct, we would expect to find occasionally an unsubmerged remnant of land (an island), accompanied with *barrier* reefs, in a region where subsidence was going on, that is, amidst a group of atolls. Now, this occurs in the Caroline Archipelago, and one or two other places. Moreover, as the conditions necessary to the life of corals (which are imperfectly known) may cease at some spots where they once existed, we might also expect (admitting the principle of subsidence) to find reefs, in which the coral being dead, could not raise itself to the low water level. Such a case is met with in the Great Chagos Bank, 90 miles by 70. It has a border from 5 to 10 fathoms under water, a second border, or inner ledge, about 16 fathoms under water, and its central parts, consisting of mud, are from 40 to 50 fathoms deep. It is conceived to be " a half-drowned atoll."

In New Caledonia, as Mr Darwin observes, we seem to witness the effects of subsidence in actual progress. It is an island 200 miles in length by 45 in breadth, quite straight, and consisting of a single ridge of mountains. Now, the coral reefs, which run parallel to its shores on the two sides, instead of turning round the north end and uniting, as we would expect, continue in their original north-west direction for 150 miles beyond it in the open sea. The most probable explanation of this anomaly is, that the reefs, in their northern prolongation, accompany a part of the ridge, which, owing to the island having subsided, is *now submarine*, but consisted of *dry land* at an earlier period when the reefs were founded. The reefs, in short, follow the ancient line of the shore, a large part of which is now under water, and the process of submergence is perhaps still going on.

Lands recently raised, or still rising from the ocean.—While ancient lands have sunk under the waves in some parts of the Indian and Pacific Oceans, Mr Darwin thinks that new lands have risen, or are rising, in others. The corals furnish the evidence of the latter change as well as the former.

As all corals are formed in the sea, it follows that when we find them *in situ* on dry land, they afford distinct proof of the land having been upraised. Now, coral banks are found in most of the Sandwich Islands many yards above the sea. In one they form three strata, each 10 feet thick. In Oahu, Mr Pierce, an intelligent European who has lived there sixteen years, is convinced that elevation is at present going on "at a very perceptible rate." Elizabeth Island (S. lat. 24, W. long. 129) 80 feet high, is entirely composed of coral. Five of the "Cook and Austral" islands (S. lat. 20, W. long. 160) are of coral *rock*. The sixth Mangaia, 300 feet high, is, with the exception of a little basalt, entirely of coral; and having a flat top with a lagoon-shaped cavity in it, is evidently an upraised atoll. Tongataboo, one of the Friendly Isles, is entirely of coral; Eoua and Vavao, in this group, the former 200 or 300 feet high, are of the same substance. Anamouka, another, 20 or 30 feet high, with a salt-water lake in the middle, is, in truth, an atoll, only a very little elevated. Savage Island, 40 feet high (south-east of the Friendly group), exhibits tree-shaped corals still unbroken, a proof that its elevation is recent. In the Navigators' group (S. lat. 14, W. long. 170) large fragments of coral were found on a steep hill at the height of 80 feet, embedded in a base of decomposed lava and sand. On the new Hebrides (S. lat. 18, E. long. 168), coral, seemingly of recent origin, is found at a great altitude. New Ireland (S. lat. 4, E. long. 153), which belongs to the Salomon group, presents beds of madreporite rock, with the corals little altered, forming a newer line of coast modelled round an ancient one. In the Mariana group (N. lat. 15, E. long. 146), a succession of cliffs of madreporite limestone present themselves. In the great circular chain of islands extending from the Bay of Bengal to Japan, embracing Sumatra, Java, Timor, Ceram, the Philippines, and Loo Choo, corals or beds of sea-shells at considerable heights, afford abundant evidence of elevation; but for details we refer to Mr Darwin's book. Where reefs occur on the shores of these islands, they are fringing reefs, indicating either that the shores are stationary, or that they are now rising.

Mr Darwin went painfully over every work in which any account of coral reefs was to be found, and marked by colours on a map to which of the three classes they belonged—of "fringing reefs," "barrier reefs," or "atolls." On classifying them in this way, the following general facts arrested his attention:—

1. They are not mingled indiscriminately, but generally those of each class appear in groups, spread over a considerable area.
2. Where they are mingled, the barrier reefs and atolls, both of which indicate *subsidence*, are found together.
3. On the other hand, fringing reefs and coral beds on *terra firma*, indicating that the land is either stationary or *uprising*, are generally found together.
4. Active volcanoes, the agents of elevation, are numerous in the stationary or *uprising* groups, and, except in a very few cases, are absent from the *subsiding* groups.

Mr Darwin was thus led to conclude that the ocean contains *areas of elevation* and *areas of subsidence;* in other words, that in some parts its bottom is sinking, and burying ancient lands under the waves; while in others, it is rising, and unveiling to us the germs of future islands and continents. Let us pursue this idea into a few details.

The Maldive and Lacadive Atolls and Great Chagos Bank, probably mark the former existence of an island extending 1500 miles from north to south, or equal in length to Britain, France, and Spain united.

In the Caroline Archipelago, northward of New Britain, we have perhaps the traces of a second island of similar size, of which two or three small portions are still above water; in the Marshall, and Gilbert, and Ellice groups, traces of a third; in the Society Isles and Low Archipelago, a few remnants of a fourth; and in the Fidgi Islands, remnants of a fifth. According to the theory also, New Caledonia and the north-east coast of Australia have subsided, and may still be subiding.

On the other hand, Sumatra, Java, Sumba, Timor, with Gilolo, the Philippines, Formosa, and Loo Choo, which abound in active volcanoes, and perhaps also Borneo and Celebes, belong to the category of uprising lands. If we suppose that the elevatory movement is still proceeding, its ultimate result, some thousand years hence, may be to unite that vast chain of islands to one another, and to the continent of Asia, by the peninsula of Malacca on the one side, and the eastern coast of China on the other, converting the Chinese sea into a vast inland lake. Further eastward, the Salomon Isles, which are also uprising, may be united into one narrow ridge, 500 miles long; and the New Hebrides, Sandwich Isles, and Navigators' Isles, may undergo a similar change. For other examples we refer to the work.

This theory explains the phenomena under consideration better than any other which has been proposed, and it is not at variance with the principles of geology, which teach us, that some parts of the crust of the globe are rising, and others subsiding at the present day. It seems to us, however, that it is attended with difficulties, of which some are perhaps apparent but others are real.

First, The anomalous facts are rather numerous. An inspection of the map shews that atolls and barrier reefs occur in "areas of elevation," and fringing reefs and volcanoes in "areas of subsidence," unless we confine these areas within very narrow limits. We grant, however, that this objection may admit of an answer. For instance, in an area that is rising, corals may take root upon a subaqueous rock or bank when it comes within less than 200 feet of the surface, and raise upon it an atoll. Again, a volcano like that of Monte Nuovo, near Naples, may break out in an area that is stationary or subsiding; and thus the indications of elevation and subsidence may be found intermingled.

Secondly, If the theory is correct, we would expect to find in areas of elevation, fringing reefs in a great variety of stages — some 2 or 3 feet above low water, some 2 or 3 yards, some with the lagoon channel almost, and others with it altogether, obliterated. That there are examples of this transition from the fringing reef to the coral rock on dry land, and that corals are found at considerable heights, we do not deny; but they occur, in our opinion, much more rarely than they ought to do, considering that the areas supposed to be uprising are of great extent, and many of them often visited and well known.

Thirdly, What seems to us the most serious objection to the theory, remains to be stated. On the outside of coral reefs very highly inclined, no bottom is sometimes found with a line of 2000 or 3000 feet, and this is by no means a rare case. It follows that the reef ought to have this thickness; and Mr Darwin's diagrams, pages 48 and 98, shew that he understood it so. Now, if such masses of coral exist under the sea, they ought somewhere to be found on *terra firma*; for there is evidence that all the lands yet visited by geologists have been at one time submerged. But neither in the great volcanic chain, extending from Sumatra to Japan, nor in the West Indies, nor in any other region yet explored, has a bed or formation of coral, even 500 feet thick, been discovered, so far as we know. We state this objection, not as conclusive against the theory, but as one deserving the able and ingenious author's consideration.

Remarks on the preceding paper, in a Letter from CHARLES DARWIN, Esq., to Mr MACLAREN.

Down near Broomley, Kent.

Dear Sir,—I have been so much pleased with the very clear, and, at the same time, in many points quite original manner in which you have stated and explained my views, that I cannot refrain from troubling you with my thanks. Your third objection appears to me much the most, indeed the only, formidable one, which has hitherto occurred to me. I fear I shall be tempted to reply to it at great length, but perhaps sometime you will find leisure to read my attempted vindication. With respect to the first objection, I can hardly admit that we know enough of the laws of elevation and subsidence to argue against the theory, because the areas of different movements are not more distinct. Some have been startled at my view on directly the reverse grounds to your objection, viz. that, according to their notions of probability, the areas of the same movements were too large and uniform. With respect to your second objection, all those who believe that exceedingly slow and gradual elevations are the order of nature, must admit a great amount of contemporaneous denudation, which would tend to annihilate the characteristic form of the fringing-reefs during their upheaval, and leave merely a coating on the upraised land of coral-rock either thicker or thinner, according to the original thickness, rate of growth of the reef at each successive level, and the rate of elevation; indeed I am surprised that there exists even one case, viz. at Mauritius, where the peculiar moat-like structure of a mere fringing-reef has been partially preserved on dry land.

Your third *criticism strikes* me as a very weighty and perplexing one.

It had passed through my head, but I had not considered it with nearly the attention it deserved, otherwise I assuredly would have noticed it in my volume. I had always intended to examine the limestone formations of England for comparison, but was prevented by bad health; I was, however, led away from the subject, and baffled when I consulted published accounts, for the limestones all appeared to be uniformly spread out, and most, if not all of them, to be associated with layers of earthy matter, whereas a formation of the nature of a group of atolls, would consist of separate large patches of calcareous rock, which would be quite pure.—I was thus led from the subject, and did not reflect on their want of thickness. The want of thickness, however, in any limestone formation, until it be first shewn to be analogous in structure, form, and composition, to a barrier-reef, an atoll or group of atolls, evidently cannot be brought forward as any argument against the theory of the long-continued subsidence of reefs of these classes. During the elevation of all reefs in open seas, I think there can be no doubt (as is dwelt on at p. 117, 3d. vol.) that a considerable thickness of the exterior would be denuded, and the only parts preserved would be those which had accumulated in lagoons or lagoon-channels; these would be chiefly sedimentary, and in some cases might contain (p. 117) scarcely any coral; within barrier-reefs such beds would often be associated with much earthy sediment. Mr Lyell, in a note just received, in which he alludes to your criticisms, speaks of the limestones of the Alps and Pyrenees, as being of enormous thickness, namely, about 4000 feet. I do not know what their composition is, but I have no doubt that the strata now accumulating *within* the barrier-reef of Australia and New Caledonia, are chiefly formed of horizontal layers of calcareous sediment and not of coral.

I suspect that denudation has acted on a far grander scale than in merely peeling the outsides of upraised reefs. My theory leads me to infer that the areas, where groups of atolls and barrier-reefs stand, have subsided to a great amount and over a wide space. Now it appears to me probable, that a subterranean change, producing a directly opposite movement, namely, a great and widely extended elevation, would be extremely slow, and would be interrupted by long periods of rest, and perhaps of oscillation of level. When I think of the denudation along the fault, which goes across the northern carboniferous counties of England, where 1000 feet of strata have been smoothed away; when I think how commonly volcanic islands, formed of very hard rock, are eaten back in cliffs from 100 or 200 to 800 or 1000 feet in height, I hardly see where we can stop, with respect to the probable limits of erosion on the comparatively soft, generally cavernous, tabular, though wide, masses of coral rock, standing exposed in great oceans during very slow changes of level. Most of the atolls which have been raised a few hundred feet are mere wrecks, and at the Friendly Archipelago where there are upraised atolls, there are large irregular reefs, also, which I have always thought were probably the basal vestiges of worn down atolls. Many submerged reefs, which may have had this same origin, occur outside the line of elevation of the Salomon and New Hebrides archipelagoes. The great steepness of the shores of upraised reefs (p. 65. Ehrenberg *quoted, and* p. *51.*) would probably be unfavourable to the growth of new reefs, and therefore to the protection afforded by them. I can conceive it very possible, that should, at some period, as far in futurity as the secondary rocks are in the past, the bed of the Pacific, with its atolls and barrier reefs, be raised in reefs, by an elevation of some thousand feet, and be converted into a continent, that scarcely any, or none of the existing reefs would be preserved; but only widely spread beds of calcareous matter derived from their wear and tear. As a corollary from this, I suspect that the reefs of the secondary periods (if any, as is probable, existed), have been ground into sand, and no longer exist. This notion will certainly at first appear preposterous; its only justification lies in the probability of upward movements after long periods of subsidence, being exceedingly slow and often interrupted by pauses of rest, and perhaps of oscillations of land, during all which the soft coral rock would be exposed to the action of waves never at rest.

This notion, preposterous as it will probably appear, would not have occurred to me, had I not several times, from independent reasons, been driven to the conclusion, that a formation to be preserved to a very distant æra (or which probably is the same thing, to be elevated to a great height from its original level *over a wide area*) must be of great extent, and must be covered by a great thickness of superincumbent matter in order to escape the chances of denudation. I have come to this conclusion chiefly from considering the character of the deposits of the long series of formations piled one upon another, in Europe, with evidence of land near many of them. I can explain my meaning more clearly by looking to the future; it scarcely seems probable, judging from what I see of the ancient parts of the crust of the earth, that any of the numerous sub-littoral formations (*i. e.* deposits formed along and near shores, and not of great width or breadth), now accumulating on most parts of the shores of Europe (and indeed of the whole world), although, no doubt, many of them must be of considerable thickness, will be preserved to a period as far in the future, as the lias or chalk are in the past, but that only those deposits of the present day will be preserved which are accumulating *over a wide area, and which shall hereafter chance to be protected by successive thick deposits.* I should think that most of the sublittoral deposits of the present day will suffer, what I conclude the sublittoral formations of the secondary æras have generally suffered, namely, denudation. Now, barrier and atoll coral reefs, though, according to my theory, of great thickness, are, in the above sense, not widely extended; and hence I conclude they will suffer, as I suspect ancient coral reefs have suffered—the same fate with sublittoral deposits.

With respect to the vertical amount of subsidence, requisite by my theory to have produced the spaces coloured blue on the map, more facts regarding the average heights of islands and tracts of land are wanted than all those, even if perfectly known, which this one world of ours would afford; for the question of the probable amount, or, which is the same thing, the probable thickness of the coral-reef, resolves itself into this,—What is the ordinary height of tracts of land, or groups of islands of the size of the existing groups of atolls (excepting as many of the highest islands or mountains in such groups, as there usually occur of "encircled islands" in groups of atolls)? and likewise what is the ordinary height of the single scattered islands between such groups of islands?—subsidence sufficient to bury all these islands (with the above exception) my theory absolutely requires, but no more. In my volume, I rather vaguely concluded that the atolls, which are studded in so marvellous a manner over wide spaces of ocean, marked the spots where the mountains of a great continent lay buried, instead of merely separate tracts of land or mountainous islands; and I was thus led to speak somewhat more strongly than warranted, of the probable vertical amount of subsidence in the areas in question.

Mr Lyell in the note alluded to, thinks we are much too ignorant of intra-tropical geology (and ignorant enough we certainly are) to affirm that calcareous rocks of the supposed thickness of coral reefs, do not occur. I am inclined to lay considerable stress on this. I do not expect the foregoing view will appear at all satisfactory to any one besides myself,—I believe, however, there is more in it than mere special pleading. The case, undoubtedly, is very perplexing; but I have the confidence to think, that the theory explains so well many facts, that I shall hold fast by it, in the face of two or three puzzles, even as good ones as your third objection. Believe me, my Dear Sir, yours very truly,

CHARLES DARWIN.

Description of an improved Tilting Apparatus for emptying Waggons at the termini of Railways, Shipping-Places, &c., as used at the Magheramorne Lime-Works, Ireland. With a Plate. By JAMES THOMSON, Esq., F.R.S.E., M.R.I.A., F.R.S.S.A., Civil Engineer, Glasgow. Communicated by the Royal Scottish Society of Arts.*

The apparatus may be generally described as consisting of three parts, viz:—

1*st*, The cast-iron brackets or quadrants for supporting the machine, Plate I. *a a a*.

2*d*, The tilting-frame upon which the waggon is placed, *b b*,—and

3*d*, The malleable iron-swings for suspending the frame to the brackets, *c c*.

The supporting brackets *a a a*, are bolted to the wooden frame *d d*, of a moveable shipping platform, by means of which the apparatus is advanced at pleasure, and made to project beyond the wharf so as to discharge the waggon immediately over the hold of a vessel.

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* Read before the Royal Scottish Society of Arts, and working model exhibited, 10th January 1842, and the Society's Honorary Silver Medal awarded, 14th November 1842.

The tilting-frame is formed of two cast-iron cheeks or sides, as shewn in fig. 4, having in each two slots or grooves for attaching to the swings, and for adjustment of the apparatus. These sides of the frame are connected together by two flat malleable iron stays *e e*, as represented in fig. 3, with two bolts in each end, and a light round iron stay *f*, at the curved ends.

The swings are attached to the frame by means of snubs *g g*, which are bolted vertically to the lower ends of the swings and horizontally to the sides of the frame, the bolts passing through the grooves or slots already mentioned, in which they are moveable—the upper ends of the swings work upon malleable iron journals fastened in the top of the cast-iron brackets. When the apparatus is properly adjusted (which is done by moving the tilting-frame forward or backward upon the swings by means of the adjusting slots), the waggon, on taking its position, should be so placed that its *centre of gravity may be slightly in advance of the point of suspension.*

The rails to the tilting-frame are laid with a gentle declivity, so that the waggon may be brought upon it with a slight impetus just sufficient to set the frame in motion—the waggon will then immediately fall into a position ready to discharge, as shewn in fig 2, when by a simple contrivance, which may be effected in various ways, the door of the waggon is opened from behind by a handle and connecting-rod communicating with the door-latch, and the load discharged.

While loaded, the position of the waggon will of itself remain the same, being in equilibrio; but immediately after it is discharged, and consequently the *centre of gravity thrown behind the point of suspension*, the tendency of the waggon is then to resume the horizontal position, which, however, it is prevented from doing, by means of the spur *h*, until completely emptied —the spur is then disengaged, and the waggon resumes its level position ready to be removed.

The whole operation of discharging a waggon (of whatever weight) is effected with perfect safety and facility in a few seconds, and one very important desideratum is supplied by this apparatus, viz. :—the practicability of *discharging waggons of different dimensions and different sized wheels upon the same tilting-frame.*

The advantages of the apparatus have been fully tested at the Magheramorne lime-works in Ireland, where they were first applied, and have since been in constant operation for the last three years, discharging waggons of three tons with 24-inch wheels, and waggons of only 20 cwt. and 20-inch wheels, with perfect facility and expedition—the cost of each apparatus not exceeding from £10 to £11 complete.

REPORT *of the Committee of the Royal Scottish Society of Arts, on Mr Thomson's Tilting Apparatus for loaded Waggons.*

Before the termination of the last Session, your Committee held a meeting to consider the merits of Mr Thomson's improved tilting apparatus, and though they were well satisfied with the principle on which the apparatus is constructed, in so far as could be judged from the drawings, yet your Committee deeming it expedient that they should have a report from the persons using the machine in the locality named by Mr Thomson, deferred coming to a conclusion on the subject, and submitted an interim report. Upon this they were instructed to correspond with such persons at Magheramorne as might be considered qualified to give an opinion of the working of the machine.

A correspondence was accordingly opened with Mr Maxwell, manager of the lime-works at Magheramorne, and the accompanying letter, dated 21st October, contains Mr Maxwell's report of the practical working of the apparatus.

Your Committee have, therefore, now no hesitation in giving a very favourable opinion of Mr Thomson's improvements on this tilting apparatus, and they are the more strongly induced to report thus favourably, from having lately learned that the improved apparatus is now being introduced upon the coal-wharfs of the Monkland and other canals; and it is, therefore, humbly suggested, that Mr Thomson merits the marked approbation of the Society. All which is humbly reported by your Committee,

JAMES SLIGHT, *Convener.*

Edinburgh, 28*th October* 1842.

MAGHERAMORNE, 21*st October* 1842.

SIR,—I am in receipt of your favour of the 17th inst., making enquiry in regard to Mr Thomson's tilting machine, and in reply, I am happy in the opportunity of bearing testimony to the great value and usefulness of the invention. Five of them were erected at our works here, about five years ago, and have been in constant and daily use since, and nothing could be more admirable than the ease and simplicity with which they work, or the perfect manner they answer the purpose for which they were intended, and in that time, without any of them requiring the replacement of almost a single bolt. Altogether I have seen no apparatus of the kind so well adapted for loading vessels with coals, limestone, or other articles of a similar heavy description.—I am, Sir, your obedient Servant.

THO. MAXWELL.

JAMES SLIGHT, Esq., Edinburgh.

Description of the Elaps Jamesoni, a New Species of Serpent from Demerara. By THOMAS S. TRAILL, M.D., F.R.S.E., M.W.S., &c. Communicated by the Author.*

THIS very elegant serpent was received from Demerara many years ago, with a collection of other snakes; and appears to have hitherto escaped the researches of the naturalists who have published on the animals of Equinoxial America. I have lately examined it anatomically, and find it provided with true moveable fangs, and with a gland, not granular, like the salivary glands of innocuous snakes, but very much resembling that of our viper, covered with an albugineous tunic, and sending a small but distinct duct to the root of its fangs. Not having met with a description of this species in any work on ophiology, I consider it as an undescribed species, and propose naming it in honour of the distinguished Professor of Natural History in this University.

The general form of this serpent, and length of its tail, approximate it to the genus *Coluber* of M. Schlegel; its physiognomy to his genus *Lycodon;* but its fangs, the whole structure of its mouth, and the fossulæ in its nasal plates, indicate that t belongs to the genus *Elaps*. Perhaps it might form the type of a new genus of venomous serpents; but unless other species resembling it be hereafter found, it is better to avoid the multiplication of genera,—the rage for which has too often greatly retarded the study of Natural History. I have, therefore, considered it as an *Elaps*, and beg leave to designate it

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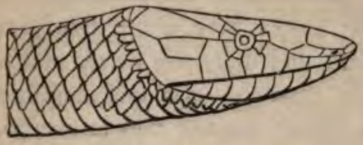
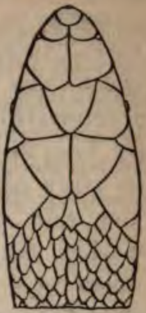
* Read before the Wernerian Natural History Society, Dec. 10. 1842.





Elaps Jamesoni.

The only specimen which I have seen, and which is in my possession, measures

	Ft.	Inches.
From the snout to the anus, ...	=4	6
From the anus to the extremity of the tail,	=1	7.5
Extreme length, . .	=6	1.5
Circumference of the trunk where thickest,	=0	4.5
Length of the head, ...	=0	1.3

The trunk diminishes towards the neck and tail. The back is slightly carinated; the abdomen is large; the tail tapers gradually; the scales are lozenge-shaped, smooth, and arranged in fifteen rows; the scuta are wide, and number 220 + 108 (the first, as in Schlegel's work, indicating the abdominal, the latter the divided caudal scuta). The general colour of the upper part of the animal is of a bluish-grey; but where the epidermis has peeled off, the scales are of a brilliant sky-blue. Each scale on the posterior part of the body, and also on the whole tail, is edged with deep black; and on the latter they are, moreover, tipt with the same colour, giving a very elegant appearance to this snake. The general colour of the under parts of the body is yellowish-white, but the abdominal scuta near the anus have their posterior edges black, and the divided scuta of the tail are deeply edged with the same hue. The plates protecting the head are nine, of the normal shape; the vertical plate is middle-sized; the temporals are rather large; the occipitals very large; the posterior frontals are considerably larger than the anterior pair; the superciliaries are large; the rostral is rounded, and emarginate below; each nasal plate has a sulcus, in which are placed the open, lateral nostrils; the frenals are wanting. There are four posterior and three anterior orbiter plates. There are eight superior and ten inferior labial plates.

The eye is rather large and prominent; the pupil orbicular.

The fangs are slender, and have a distinct longitudinal furrow on their anterior convex surface, as in Schlegel's first subdivision of venomous serpents. They are attached to the maxillary bones, which are, as usual in venomous snakes, moveable by muscles attached to the pterygoid bones. The poison-gland, placed at the angle of the jaw, is covered by a firm albugineous tunic, has a cellular structure, and sends off a slender poison duct, in the usual manner, to the root of the fangs.

These particulars are noticed to shew that this serpent really belongs to the true venomous snakes, not to the *Lycodons*, with which a superficial view might readily confound it, as it has several analogies with that genus of harmless serpents.

Edinburgh University, *March* 19. 1842.

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On the Application of the Hypothesis of M Venetz to the Erratic Phenomena of the North; a Letter addressed to M. Macaire, Counsellor of State, by M. Jean de Charpentier.[*]

Sir—You have been good enough to take the trouble of giving in the *Bibliothèque Universelle de Genève* an account of my *Essai sur les glaciers et le terrain erratique du bassin du Rhône.*[*] I there remarked with much satisfaction that you have perused that work with attention, and have completely understood the ideas which it was my intention to express. In fact, I think it would be impossible to prepare a better abstract than you have published of a work which, in some measure, is only a summary of observations. I therefore request you to accept of my sincere thanks on this account, and also for all the kind observations regarding me, which, on that occasion, were dictated by your indulgent goodness.

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[*] From the Bibliothèque Universelle de Genève, No. 78. As we have all along endeavoured, so far as our space permitted, to convey to our readers full information respecting glaciers, and the topics more immediately connected with them, we collected, at p. 160 of vol. xxx., references to the most important papers which had appeared in this Journal on the subject; and we now continue that list, premising the titles of some shorter

If all my readers had considered the subject with the same attention and the same sagacity which you have brought to

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communications published previously to vol. xxx., but not included in our former note : Vol. xviii. p. 363, Klöden on the Origin of the Erratic Blocks of the North of Germany. Vol. xxiii. p. 69, Sefström on the Traces of a vast Ancient Flood (On *ösars* and *Jättegryttor*). Vol. xxiv., p. 438, Von Baer on the Transported Blocks of the South Coast of Finland. Vol. xxix. p. 135, On the Origin of Fissures in Glaciers, and on Sefström's Investigations. Vol. xxx. pp. 160 and 284, Dr Martens on the Glaciers of Spitzbergen, compared with those of Switzerland and Norway ; p. 194, Dr Buckland on the former existence of Glaciers in Scotland ; p. 199, Mr Lyell on the Geological Evidence of the former existence of Glaciers in Forfarshire ; p. 202, Dr Buckland on the former existence of Glaciers in the North of England. Vol. xxxi. p. 38, Dr Black on the Antediluvian Congelation of the Interstitial Water of Rocks ; p. 56, Captain Vetch on Icebergs, &c. ; p. 77, M. Renoir on the Traces of Ancient Glaciers in Dauphiny and in Northern Russia ; p. 252, M. Robert on the Grooves and Furrows on the Rocks of Scandinavia ; p. 253, M. Böhtlingk on the Traces of the last Revolution in Scandinavia. Vol. xxxii. p. 76, Professor Hitchcock on Glacial Action, &c., in America ; p. 84, Professor Forbes on a Remarkable Structure observed by him in the Ice of Glaciers ; p. 103, M. Böhtlingk on the Scratches and Furrows observed on the Rocks of Finland ; p. 291, M. Desor's Account of an Ascent of the Jungfrau. Vol. xxxiii. p. 1, Sir G. Mackenzie on an Hypothesis to account for the Origin of Glaciers ; p. 36, Professor Bronn on the Glacier Theory of Agassiz ; p. 104, M. de Charpentier on the Glaciers and Erratic Formation of the Valley of the Rhone ; p. 124, Mr Murchison on the Glacial Theory ; p. 161, M. Studer on the Diluvium and Erratic Blocks of Switzerland ; p. 217, Professor Agassiz on the Glacial Theory and its Recent Progress ; p. 338, Professor Forbes' Recent Observations on Glaciers ; p. 352, Mr Darwin on the Ancient Glaciers of Caernarvonshire ; p. 399, Professor Agassiz' Recent Observations on the Glacier of the Aar.—Edit.

[*] Jameson's Journal, vol. xxxiii. p. 104.

bear on it, the hypothesis of M. Venetz, that is to say, the hypothesis which attributes the transport of erratic blocks to glaciers, would certainly by this time have gained a larger number of supporters. There are, it is true, many persons who adopt it for the explanation of the erratic phenomena of the Alps; but this is not the case with regard to the erratic phenomena of the north of Europe. Nevertheless, there seems to me to be so great an analogy between the erratic phenomena of the north and those of the Alps and the Pyrenees, that we may assert that there is an almost complete identity. Not having visited any of the countries of the north, I only know the erratic phenomenon of Scandinavia by the descriptions that have been given of it, but the most interesting of these had not appeared, or at least had not come under my notice, before the publication of my book. Judging from the descriptions given by skilful observers and good geologists, the difference between the erratic formations of the north and those of the south, consists solely in the extent of the dispersion of the debris; that dispersion being in the north spread over a surface incomparably greater than in the south. It appears, moreover, that in the north, floating masses of ice have had a share in producing this dispersion, whereas in the south, such an agent has been so feeble in its operation, if it existed at all, that traces of its action have not yet been ascertained.

Although I am far from pretending that analogous, or even identical facts, are always the result of a common cause, it seems to me that the glacier hypothesis explains the erratic phenomena of Scandinavia quite as well as it does those of the Alps. The great repugnance which has hitherto been shewn to the application of this hypothesis to the transport of the erratic debris of the north, proceeds, 1*st*, From the false idea that has been adopted of the mode of formation, the development, and the movement of glaciers; and, 2*d*, From the error of believing that the glacier hypothesis excludes all operation of other agents.

Notwithstanding the care I took in the first part of my book to describe, as clearly as was possible for me, the chief phenomena of glaciers, and to explain their theory, it nevertheless appears that I have not always been properly understood, for there are still many persons who never hear the word glacier, without associating with it the idea of mountains, lofty mountains, mountains of many thousand feet in height. Such individuals think that mountains are an indispensable condition for the existence of glaciers; but such an opinion is quite erroneous. Mountains do not exercise any direct influence on glaciers, except that they sometimes favour the accumulation of snow drifted by the wind. It is only their cold, snowy, and rainy climate which causes the formation, development, and movement of glaciers. Now, then, if from any cause a similar climate existed in a flat country, were it even at the level of the sea, there would be nothing to prevent glaciers from being formed and developed. Nor is the declivity of the surface a necessary condition for their movement; for, as I have shewn in my *Essai* (§ 14), glaciers do not move by the action of their own gravity, nor by the pressure of the high *nevés*, or upper snow; this movement being produced solely by the dilation which the ice undergoes, when the water that it has absorbed by means of the capillary fissures traversing its whole mass, becomes frozen. Consequently, if a cold, snowy, and rainy climate existed during a long course of years in a region forming part of a flat and smooth country, and if the summer temperature were insufficient to cause the complete melting of the winter snows, these snows would not fail to be converted into glacier. If the surface of that region presented a perfectly horizontal plane, the glacier, as it became developed, would extend in the direction of rays from the centre to the circumference; but if the surface were inclined, that extension, and consequently the principal movement, would take place in the direction of the line of greatest inclination (*Essai*, § 22). These considerations render it apparent, that the absence of high mountains, and the presence of immense plains, in countries where the erratic debris of the north have been met with, cannot furnish a valid objection to the glacier hypothesis.

The change of climate supposed by the hypothesis, must have occurred after the great catastrophe which has modified the surface of an immense extent of the northern hemisphere, and has given to the principal chain of the Alps, to the Atlas group, to the Caucasus, to the Himalaya, &c., their present configuration.[*] It must have been the effect, the inevitable consequence, of that revolution (*Essai*, § 82). The facts demand this conclusion in so decisive a manner, that it is even admitted by geologists who do not adopt the glacier hypothesis. Thus M. Durocher[†] supposes, "that the winters in Europe were colder during the geological period which immediately preceded the present one;" that is to say, the epoch during which the dispersion of the erratic debris took place. This opinion is supported in a note at the bottom of the page by M. Elie de Beaumont. Instead, however, of supposing with M. Durocher, the existence of colder winters than those of the present day, I should rather be inclined to believe that they were more snowy than they now are, but that the summers were more rainy and colder, so that the difference between the mean temperature of summer and that of winter was less considerable than it is at present. Such a climate must have been very analogous to that of Terra del Fuego, and the northern coast of the Straits of Magellan; for, judging from the work of Mr Darwin,[‡] the climate of the most southern portions of America is perfectly similar to that which must formerly have prevailed in the north, if the summers in the former were a little more cold and more rainy, and the winters more snowy. If this were the case, these regions would now present us with the same phenomenon which was formerly exhibited in the north, that of a vast country entirely covered by an immense glacier.

There is another difficulty which prevents many persons from adopting the hypothesis of glaciers for the explanation of the erratic phenomenon of the north, a difficulty arising solely from the erroneous idea conceived of the origin of the snow or the ice that must have formed that immense glacier. They, in fact, imagine, that the snow which has formed the ice of a glacier, proceeds entirely from the mountain on which it takes its origin; and they found this opinion on the fact, that they find at the foot of the glacier, among the debris brought down by it, fragments of rock evidently detached from that mountain. Starting with this idea, they believe that the hypothesis in question, applied to the erratic phenomenon of the north, obliges them to admit that the ice which formerly covered the countries where the erratic debris are met with, that is to say, the immense extent included between the north of Scandinavia, Moscow, and Leipsic, came wholly from the mountains of Norway or of Spitzbergen, or of some part of the Polar regions. But such a supposition is quite as inadmissible as that which would attribute to the source of the Rhone all the water which that river contains when it falls into the lake, and that because there had been recognised among the wood it transports, trees evidently derived from near its source. The absurdity of this supposition, though based on a fact which is very true, is at once apparent. It is the same thing with glaciers; for the snow which has given rise to the formation of the ice, does not all come from the mountains where they had their origin; on the contrary, the ice derived from the *hauts-névés* (*Essai*, § 3 and § 10) only forms a part, sometimes a very small one, of their entire mass. In fact, as the ice of a glacier is chiefly produced by the congelation of the water which, as often under the form of rain as of snow, has fallen on it and been absorbed by it, it is evident that the more surface a glacier presents, the more the portion of ice having that origin ought to be considerable, compared with that which has really descended from the mountain. There is therefore no need of supposing that all the ice of the diluvian glacier of the north came from one single point; on the contrary, that vast glacier would be constantly increased by the rain and the snow falling directly upon it, and its increase must have gone on augmenting in proportion as it acquired a larger surface.

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[*] Elie de Beaumont in the French translation of De la Bêche, p. 659.

[†] Report on a Memoir by M. Durocher, entitled, *Observations sur le Phénomène Diluvien dans le Nord de l'Europe*, p. 25. (*Comptes Rendus*, vol. xiv. p. 101, Edit.)

[‡] *Journal of Researches in Geology*, &c.

We must no longer persuade ourselves that the change in the snows of the north only commenced its operations at one single locality, more or less limited. This change must have taken place simultaneously in the whole region where the summer temperature was not sufficiently high to cause the entire disappearance of the winter snows. Such a state of the climate must have extended over a large surface, which must have comprised, as we shall immediately see, Finmark, Lapland, Norway, and the greater part of Sweden and Finland. Consequently, a glacier formed at once over so large a surface, must, in a short time, have acquired an immense development. As it crossed the Baltic and extended to the north of Germany, Prussia, and the plains of Russia, as far as Moscow, there is nothing extraordinary in supposing that the erratic formation really reaches to Moscow, Stezyka, Oppeln, Leipsic, &c., and that in the indications of the boundary of this formation, it may sometimes have been regarded as identical with the diluvium, as I am almost tempted to believe.

These considerations shew us that the supposition of a glacier occupying nearly the whole of Scandinavia, and stretching over a portion of the countries situated to the south of the Baltic, does not imply any thing impossible or contrary to the laws of physics. The only thing that may appear at first sight a difficulty, is the circumstance, that this glacier must have traversed the Baltic and its gulfs, and that sea must undoubtedly have been, at the period alluded to, of much greater extent than it now is. But what I have said in my Essay (§ 305) regarding the lakes which occurred in the course of the diluvian glaciers of the Alps, is equally applicable to the sea; while the localities where there were no currents of an elevated temperature, like the Gulf Stream, could not have been an obstacle to the progression of a glacier of such vast breadth as the diluvian glacier of the north.

The erratic formation presents itself in the north under the same form as in the Alps, and exhibits the same phenomena. Thus, the debris of the rocks are sometimes scattered widely, which is most frequently the case, and sometimes accumulated in bands or mounds. Fragments of all sizes are met with mixed pell-mell, without any separation, according to their volume. Many of them have their prominent portions well preserved, as well as their surfaces. The rocks, as in the Alps, exhibit marks of wearing and rubbing, smooth surfaces, striæ, furrows, and vertical erosions in the form of cauldrons.

Deposits of diluvium are likewise met with, composed of beds of pebbles, of sand, and of mud, and not only within the limits of the erratic formation, but also beyond them, at a great distance to the south. The Scandinavian diluvium, indeed, covers a considerable extent of the north-west of Russia, of Prussia, of Poland, and of the north of Germany. This formation, whose materials have evidently been transported and deposited by water, offers a feature which has not yet been observed in the Alps, and that consists in the presence of well-preserved angular debris, and of large blocks, beyond the domain of the erratic formation. The good state of preservation of their surface, of their angles, and of their edges, as well as the considerable volume of a large number of them, do not allow of their being regarded as having been transported by water. It is therefore to be presumed, that their transport was effected by floating ice. The external configuration of the region in which the glacier had its origin, and that of the countries successively invaded, far from being unfavourable to this supposition, render it, on the contrary, very probable. In fact, the masses of ice which must from time to time have been detached from the glacier, and carried away by the water, had not, as in the Alps, to cross narrow defiles, or to follow valleys with numerous windings, in which they would be speedily broken up against the mountains forming the re-entering angle of the bend.

The marine shells frequently found in the diluvium, prove that, at the epoch of its formation, the countries where they are observed must have been submerged by the sea. The perfect preservation of these molluscous animals, belonging chiefly to species still living in the seas of the north, and the stratification, often very regular, of these sedimentary deposits, do not allow us to doubt that the materials were transported by slow currents, or, at all events, by currents of but little rapidity.

But those who reject the glacier hypothesis, and wish to explain the erratic phenomena of the north solely by floating ice and currents, fall, in my opinion, into great improbabilities. First of all, in order to explain the marks of rubbing and of wearing on the rocks, they are obliged to commence with the supposition of an enormous current, flowing from north and south, and for whose origin they have to seek "to the north of Scandinavia, perhaps even beyond Spitzbergen and the neighbouring islands, towards the polar regions."* In order to account for the facts, it is absolutely necessary to admit that this current, like a flowing tide, had risen on the coast of Finmark to the height of 2500 feet above the present level of the sea, because it is at that elevation, on the summit of the mountain of Raipas and on the high plateau of Norwegian Lapland, that M. Durocher found polished and grooved surfaces of rock. But pure water cannot polish and scoop out rocks; and we are thus farther constrained to admit, on this hypothesis, that the current was charged with matter from the bottom of the sea to the height of 2500 feet above its present level.

I confess I cannot conceive what catastrophe could have produced such a current, a tide so monstrous; nor can I imagine the current itself, especially when I consider that this mass of water could not be confined between the mountains of a valley, but that it must have been accumulated on an open and boundless sea. The supposition of the *soulèvement* of an island, of a vast island, even of a continent, does not explain to me, in a satisfactory manner, this enormous current. If the *soulèvement* was gradual, it could not occasion rapid currents, and still less so great an accumulation of water on the surface of the sea. We must, therefore, suppose that this *soulèvement* was as sudden as the explosion of a mine; but a sudden and instantaneous *soulèvement* seems to me the least probable occurrence in the world.

But leaving aside the difficulties arising from the cause and the mode of formation of this current, let us suppose it to have been such as is required by the hypothesis, that is to say, endowed with great rapidity, and charged with materials for rounding rocks, polishing surfaces to a height of 2500 feet, and forming those accumulations of debris in the form of mounds or causeways, known in Sweden by the name of *ösars*. In this case, I would ask, What has become of these materials? Have they, perhaps, been all employed in the construction of the ösars? That cannot be, because the total mass of these accumulations is much too small compared with the quantity of rocky debris which the current must have transported. Perhaps this excess, this surplus of materials, may have given rise to the deposit of diluvium which is of such extent in the north of Europe? But neither could that be the case, for the stratification, often very regular, of this formation, and the good state of preservation of the shells which it contains, do not allow us to attribute its formation to a current so sudden and so impetuous as that one must have been which is supposed to have abraded and furrowed the rocks, and to have transported the blocks constituting the ösars. How did these matters not fill up, if not the Gulfs of Scandinavia, at all events the lakes existing in such abundance in the countries invaded by this debacle? I am indeed unable to give a reply to this question.

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* M. Durocher, *Mémoire*, p. 32. (*Comptes Rendus*, vol. xiv. p. 108. Edit.)

Perhaps an objection to the glacier hypothesis will be found in the quantity of debris composing the erratic formation, for it may be said, and with much reason, that the mountains which rose above the surface of the glacier were too few in number, and presented too limited a superficies, to allow of the eboulements which fell on the glacier, furnishing a mass of debris so considerable as that now found distributed. This objection would, indeed, be unanswerable, if the materials which a glacier transports must necessarily have *fallen* on its surface. But it is not so, for the fragments of rock which we find on the ridge of a glacier are not all derived from eboulements; on the contrary, there are many of them which come from the bottom or bed of the glacier. As to the manner in which these stones arrive at the surface from the bottom or bed of a glacier, I have described it in detail in my Essay (§ 25). Thus, then, undoubtedly, the largest portion of the debris constituting the erratic formation and the diluvium of the north, does not owe its origin to eboulements. These fragments have been detached from the rocks at a period anterior to the formation of the ice, by the very revolutions which varied the configuration of Scandinavia, and they have arrived at the surface of the glacier, not from above by a descent, but from beneath, having been elevated by the ice.

The external configuration of the ösars, "being in the form of long mounds," is, in my opinion, much better explained by the glacier hypothesis, than by that of a great current and of floating ice.

It is the same with the fine striæ which have been engraved on the surface of the rubbed and smoothed rocks. If currents, transporting matter, could produce striæ of this description, these ought also to be met with on the naked rock of the beds of torrents, where, however, we never find them.

The hypothesis of currents and of floating ice is altogether insufficient to explain the vertical erosions, having the form of caldrons, so common in Scandinavia, where they receive the name of *Jättegryttor*, (*Riesentöpfe*, in German) or giants' boilers.* There is, in fact, no other hypothesis but that of glaciers, which can account in a manner really satisfactory for this remarkable phenomenon (*Essai*, § 35 and § 80.)

If I were not afraid of exceeding too much the limits of a letter, I could adduce other improbabilities and other difficulties which present themselves, when the whole erratic formation of the north is attributed *solely* to an enormous current, and to floating ice. I will do this when I continue (according to my announcement, *Essai*, preface, p. 10), my work on glaciers and the erratic formation. I shall then shew that this astonishing phenomenon can be explained even to its most minute details by the hypothesis of glaciers, combining it at the same time with that of floating ice and currents. I must, however, state, that by *currents*, I do not mean that debacle, that enormous tide, which must have reached a height of 2500 feet above the level of the sea, and which I cannot admit; but I suppose the existence of currents similar to those of the seas of the present day, and to the great rivers of flat countries.

If we admit the combination of these three causes, against which no valid objection can be made, we shall be able to explain the dispersion of the erratic debris of the north in as satisfactory a manner as we can that of the Alps and of the Pyrenees. It will be the task of the geologist to assign approximately the share which each of these agents has had in the production of this great phenomenon.

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* Bergmann's *Physikalische Beschreibung der Erdkugel*, vol. ii. p. 193; and Sefström, in Poggendorff's Annalen, vol. xxxviii. p. 614, and in Jameson's Journal, vol. xxiii. p. 69.

The deposits of erratic debris, properly so called, the abrasion of the rocks, the marks of attrition, the striæ, the furrows, and the erosions in the form of caldrons, are to be attributed to glacier action. Erratic deposits can always be distinguished from the diluvium by the frequency of well-preserved angular debris. The *ösars* serve not only to prove the existence of the erratic formation in any particular region, but are also of great assistance in determining its limits. For this purpose it would be necessary to delineate on a map the ösars the farthest removed from the north, or, in their absence, to indicate the localities where the debris cease to be mixed pell-mell as regards their volume, and where, consequently, a selection, according to relative weight, begins to be perceptible.* The line joining all these localities would indicate the limit of the erratic formation properly so called, that is to say, the limit of the debris dispersed by the glacier. It would also exhibit the form of the glacier at the period of its greatest development. Consequently, the regions comprised between this line and the north, must have been covered by ice at the epoch of its maximum of extent.

The sedimentary deposits, whether stratified deposits of pebbles, of sand, or of clay, situated within or without that line, are, in my opinion, not the erratic formation, but diluvium, that is to say, a sediment whose materials have been conveyed and deposited by water. In the countries which were not submerged by the sea, this transport must have been effected by the streams which issued from the glacier, and which, during the period of its melting, doubtless acquired a considerable volume. But in regions covered by the sea, this transport could only have been effected by an actual submarine current, produced by the difference of temperature between the water in the vicinity of the glacier, and that which was more to the south. Looking at the course of the current, it must have assumed a direction from north to south, and traversed the bottom of the sea; the greater part of that sea having probably had but little depth. Carrying along with it the comminuted debris, that current deposited the diluvium which constitutes the plains of the north-west of Russia, of Poland, of Prussia, and of the north of Germany.

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* It will be found that there is rarely an opportunity of observing marks of attrition in the vicinity of the limit of the erratic formation, because the regions where it terminates being in the plains, the rock constituting the surface is generally covered and masked by the diluvium.

Beyond the limit of the erratic formation, and dispersed on the surface of the soil or enveloped in the diluvium, we find fragments of rock which have their surfaces and their prominent portions in a good state of preservation. Rolled blocks are also met with there, whose volume is too considerable to allow us to suppose that they have been transported by currents, which, judging from the regularity of the stratification of the diluvium, cannot have been violent. I attribute, without hesitation, the transport of such matters to floating ice, that is to say, to masses of ice detached from the glacier, of which some have been transported by rivers, while others, and probably the larger proportion, having fallen into the sea, have been forced to the south by the impulsion of the winds from the north; in fact, marine currents could not have conveyed them to the south, because, that of the bottom having pursued, as I have already said, a course from north to south, the current existing at the surface must have had a contrary direction.

The formation of the erratic formation must have commenced from the period when the snow was transformed into ice. But these first deposits were not permanent; for in proportion as the glacier made progress, it overthrew them and displaced them anew. It thus continued to destroy its own work until it reached the *maximum* of its development. During the time of its greatest extension, it formed the terminal moraine, that is to say, the moraine farthest to the south. The circumstance that this moraine probably does not exist along the whole line indicating the shapé of the glacier at the period of its greatest development, cannot be an objection to the hypothesis which I defend. In fact, existing glaciers themselves are not uninterruptedly skirted by moraines; for the latter cannot be met with except in the localities where the rocky debris have reached the edge of the glacier, and where torrents have not prevented its accumulation. Moreover, in places where the glacier deposited little matter, the moraine having remained small and but little elevated, has been afterwards buried by the diluvium, and thus removed from the view of the observer.

On each occasion when the glacier, during the process of melting, was subjected to some oscillation, it gave rise to new accumulations of debris. In this manner it necessarily formed other frontal moraines; these are recognised by their direction, which is nearly east and west, and they are known in eastern Prussia by the name of *Steindämme*.

Having at last retreated beyond the Baltic, the glacier was so much reduced as only to occupy the regions in which it had originated. The return of a milder climate must also have gradually produced a melting in these countries. We can easily conceive that the lower regions were the first that were freed from ice; but that the latter kept its ground on the mountains and higher table-lands, until the return of heat had also reached such elevated points. Previous to this complete melting, the glacier was, so to speak, lacerated or divided into shreds, forming so many separate glaciers, of which the largest, as happens in the Alps, descended into the neighbouring valleys, and, depositing on the flanks of the mountains the debris which they transported, caused the formation of the Scandinavian ösars of the present day. When the mountain which retained its ice, was more or less isolated, or advanced into the flat country, so that the glacier which descended from it could extend freely over a smooth surface, there would result the phenomenon described by M. Durocher, and which consists in this, "that in taking each of the rocks which have furnished erratic blocks as the centre of a circle, the region which contains blocks derived from that rock, occupies more than a third, and sometimes nearly a half, of the circumference, so that the blocks have followed, in certain cases, a line almost perpendicular to the general direction which the power of transport from north to south ought to have" (*Mém.* p. 17.) I quote this fact, because it explains extremely well the crossing of the striæ which is sometimes remarked on the surface of rocks. The localities where the striæ cross have been covered at two different times by ice; the first time, they have been invaded by the great glacier, which has scratched them in the general direction from north to south; and the second time, they have been so by partial glaciers, whose action has there produced striæ, in some degree anomalous, which cross the first in various directions.

When two of these partial glaciers became joined together and united into a single one, they would give rise to a superficial moraine. (*Essai*, § 20 and § 21.)*

The abraded and polished surfaces of rocks, the striæ, furrows, and caldron-like erosions, could only be produced during the period when the various localities where they are observed were covered by ice. The direction of the furrows and striæ being generally from north to south, we are authorized in believing that the principal movement of the glacier was in that direction.

In order to assign the cause which determined this direction, we must turn our attention to the state of the snow in the north during the epoch of which we are speaking. I have already remarked that the whole erratic phenomenon obliges us to admit that some time after the last great catastrophe which altered the configuration of the northern hemisphere, the climate became so much colder, that in Scandinavia, perhaps from the 60th degree, the summer heat was no longer sufficient to cause the complete melting of the winter snows. Nevertheless, the liquefaction was not entirely suspended, and the water proceeding from it, as well as that derived from rain, must gradually have converted the snow into a glacier which invaded countries more to the south and having a milder climate. But it is probable that from the 70th degree the melting of the snow had nearly ceased, or, at least, that it was scarcely more considerable than it now is on our most elevated mountains. The snow, beyond the 70th degree, from the impossibility of its transformation into glacier, must have corresponded completely with the most elevated *hauts-nevés* (*Essai*, § 3). The fact that the larger portion of the polar regions is occupied by seas, is not opposed to this supposition; for, if these seas, as is very probable, were then covered by ice, as they are at the present day from the 80th degree, the snow could rest there just as well as on solid land.

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* The ösars which have had this origin may be recognised by the fact that their upper extremity generally rests against the rock or eminence forming the termination of the chain of mountains, which, by separating the two glaciers, has given rise to the deposit of the superficial moraine (*Essai*, p. 55, fig. xii., c and l.) This appearance has been supposed to be an evident proof of the formation of ösars by a powerful current.

Nor is there anything which obliges us to restrict the transformation of snow into ice to Scandinavia alone. On the contrary, it is more probable that the conditions of climate necessary for the transformation were to be found in the whole zone, comprised between the 60° and 70° parallel. This supposition is supported by the existence of the erratic formation in Siberia, and in the North of America. The isothermal, and particularly the isotheral lines, have, it is true, materially modified the northern limit of this zone of permanent snow; but these modifications, however great they may have been, do not at all influence the theory of the erratic phenomena.*

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* The isothermal lines, and especially the isotheral lines, must have exercised a considerable influence on the formation and on the development of the diluvian glacier of the North. It is, without doubt, in the direction or course of these lines that we must seek for the cause of the erratic formation not reaching the same parallel throughout the whole of the north; thus, for example, the limit of this formation advances much more to the south in the north of Germany, than in Russia and in Siberia. It is plain, that the more these lines ascend to the north, the less could the glacier advance towards the south. The exact determination of the limit of the erratic formation, would be of great importance for the physics of the globe; it would throw much light on the climatological condition of the north of the northern hemisphere during the earliest periods of the present geological epoch. But in order that this investigation might accomplish its object, and acquire that scientific interest, it is indispensable that the erratic formation should be accurately distinguished from the diluvium, because, by confounding these two formations, as is often done, false results are obtained, and erroneous conclusions deduced.

We may admit, therefore, that, some time after the last great revolution of the globe, the northern hemisphere was covered by a sheet of snow, from about the 60° parallel to the pole; and that the snow of the zone comprised between the 60° and 70° parallels, was transformed into a glacier, which, in its dilatation, could not extend in any other but a southern direction, because, in other directions, it had to encounter the resistance arising from snow and ice themselves (*Essai*, § 11). The movement of the glacier must, therefore, have been from north to south. This result of the theory is completely confirmed by the observation of facts, for we know that the general direction of the furrows and scratches traced by the great glacier, is nearly in that direction. The slight deviations, sometimes remarked, have been occasioned by the slope and inequalities of the surface. There is likewise another fact, which proves conclusively that the movement of the glacier was from north to south. This consists in the fact, that the northern flank of the rocks having been exposed to the whole action of the expansive force of the ice, and to that of the movement, presents marks of abrasion and attrition of a much more distinct nature than the flank directed towards the south, which, having been more or less sheltered by the body of the mountain, must have experienced to a smaller extent the effect of this action. An argument has been drawn from this fact in favour of the debacle or great northern current; but the same phenomenon actually takes place under our eyes in the Alps, for, when a glacier encounters a rock or eminence in its passage, we find that the flank turned towards the side whence the glacier proceeds, is always more rounded and more rubbed than that turned towards the opposite direction.

Lastly, as to the formation of the diluvium, which is met with not only within the limits of the erratic formation, but also to a great distance beyond it, it must have commenced in the first periods of the epoch which we are now considering, and must have gone on augmenting in proportion as the glacier was developed. The materials which were deposited, as much by the rivers as by the submarine current, in the regions afterwards invaded by the glacier, experienced new displacements; because, as in the case of modern glaciers, that of Scandinavia must have upturned the soil, and pierced to the solid rock, in the localities where the inequalities of the formation interfered with its movement. But where it could extend freely, and where there was no obstacle to the expansion of the ice, it must have stretched over the diluvium without raising it, if, at least in the upper beds, the latter was of such a nature as to afford the water the means of flowing off quickly (*Essai*, § 16).

Although the deposition of diluvium may have been going on during the whole period of the existence of the glacier, it will nevertheless be easily understood, that the largest quantity of boulders, sand, and clay, was transported during the melting of the ice; so that, in many localities, the erratic formation must have been covered by it, especially if it only presented scattered deposits (*Essai*, § 47).

The transport of fragments of rock, by means of floating ice, must have taken place during the whole period of the existence of the glacier; but it is when the glacier was most in contact with the sea, that this transport must have been most frequent. I have already said, that I attribute to this mode of transport, the angular and well-preserved debris, and the blocks of large size, which are both found beyond the limit of the erratic formation, lying sometimes scattered on the surface of the ground, sometimes disseminated in the interior of the diluvium. The first must have been carried thither when the current and the rivers had ceased to convey matters to the locality where these fragments are found; the others, when the transport of boulders, sand, and clay, caused by currents, was still taking place.

You are now, Sir, in possession of my opinion regarding the mode of origin of the Erratic Phenomenon of the North, which, however, I have not had an opportunity of examining personally, but only know from the descriptions that have been given, and more especially those of Messrs Durocher, Böhtlingk,* and Sefström. However succinct, and therefore imperfect, may be the summary which I have now offered, of the manner in which I conceive this great phenomenon to have been caused, it must suffice, I think, to shew that the hypothesis of M. Venetz, combined to a certain extent with that of floating ice, accounts for it better than that which attributes it to an enormous current, coming from the polar regions, and which, at the same time, assigns too important a part in the operations to floating ice. This latter hypothesis, apart from the improbabilities which it presents, is, even in the opinion of its defenders, insufficient to explain many facts that are of importance, and are connected with the erratic phenomenon; it thus leaves us in doubt and in uncertainty.

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* Jameson's Journal, vol. xxxii. p. 103.

Permit me, Sir, to terminate this long letter by giving, in a few words, a summary of the principal ideas which I have now offered:—

1. In consequence of the last great catastrophe which altered the configuration of the surface of the northern hemisphere over a vast extent, the climate became colder and moister than it was previously, or is at the present day.

2. During the long continuance of this climatological condition, the summer temperature was insufficient to melt *completely* the snows from the 60th parallel.

3. The snows comprised between the 60th and 70th parallels were transformed into glaciers. Beyond the 70th parallel they remained in the state of *névé*.

4. This glacier having acquired a considerable development, invaded the north of Russia as far as Moscow, Prussia, Poland, the north of Germany, and perhaps the eastern shores of England.

5. It transported and deposited the erratic formation, and produced marks of abrasion, the striæ and furrows which have been observed on rocks. The cascades to which it gave rise have caused the erosions in the form of caldrons.

6. The most southern accumulations, having the form of mounds or bands, are the moraines which it deposited during the maximum of its development.

7. Osars are moraines, some having been formed by the oscillations to which the great glacier was subjected during its retreat, others by the ice which remained on elevated mountains and table-lands, long after the low regions had been *freed from it.*

8. The matters constituting the diluvium, both those within and those without the limits of the erratic formation, were conveyed by rivers and by the submarine current.

9. The great mass of diluvium was deposited during the melting or retreat of the glacier.

Lastly, 10. The angular debris and the blocks of large size, dispersed on the surface of the ground or embedded in the diluvium, but both beyond the limits of the erratic formation, have been transported by masses of ice, detached from the glacier. Of these masses of ice, some have been carried along by rivers, and others, floating on the sea, have been propelled towards the south by the force of the winds.

Bex, 26th May, 1842.

Fragments of Philosophy. By Sir WILLIAM HAMILTON, Bart., Professor of Logic and Metaphysics in the University of Edinburgh.*

For some years we have heard much of the Scottish and German philosophy, of the former especially, which M. Royer-Collard and M. Cousin have assisted in making known by means of their eloquent lectures; but it happens in this case, as in so many others, that the word is more familiar than the thing, and the first mentioned of these two philosophies not having yet become the fashion, it has hitherto continued in some degree of obscurity, from which it is of importance that it should be freed.

The four philosophical dissertations translated in the work, the title of which has been given above, will be fitted to throw some light on this important subject: they are from the pen of Sir William Hamilton, Professor of Logic and Metaphysics in Edinburgh. This author would have been almost unknown in France until the appearance of the work in question, had not some of our professors mentioned his writings. Messrs Barthelemy Saint-Hilaire, Cousin, and Jouffroy, have done us this service, which is undoubtedly of some value, when we consider that all his productions, published anonymously in the Edinburgh Review, are scarcely known, in regard to their authorship, even in their own country. Sir W. Hamilton is one of those profound thinkers and true friends of science, who never think of publishing their works till they conceive them to be of such a nature as to produce some solid and substantial result. It happens more frequently still, that writers of this description, thinking little of the public, are entirely occupied with satisfying the wants of their own mind; having but little anxiety about the effect of their thoughts on others, their mind dwells only on the intrinsic value of their researches, and they create for themselves, as Maine de Biran said, "a world in their own brain." By this, however, we do not mean to say that Sir W. Hamilton is a visionary or a fabricator of fantastical systems; hitherto, on the contrary, his career has been one of remarkable activity; but the pledges which he has given to science rest almost entirely on the merits of his teaching; his publications, hitherto few in number, bear the impress of true and original powers of mind. The four dissertations collected in this volume, have been selected from the pieces which the author has laid before the public; these pieces altogether do not exceed the amount of a dozen articles, but all afford proofs of a rich philosophical erudition, and an excellent method of investigation. Convinced as we are that the Scotch philosophy is not yet truly known in France, we do not hesitate to offer a succinct analysis of the fragments translated in Mr Peisse's volume; it will be the means of familiarizing our readers with this philosophy, which ought not to be strange to us, and also of rendering homage to a modest and laborious philosopher. But, before entering upon the examination of the volume, let us supply some particulars regarding the author.

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* *Fragments de Philosophie, &c.* Translated, with a long Preface, Notes, and Appendix, by L. Peisse. Paris, 1840.

Sir William Hamilton belongs to the great family of Hamilton, which has given to France one of its classical writers. He commenced his studies at the University of Glasgow, and concluded them at Oxford. Having acquitted himself with honour in the examinations requisite for obtaining University degrees, he entered himself at the bar, obtained the chair of Universal History, and subsequently gave up this charge for another more suited to the nature of his talents and the character of his studies. Thomas Brown died in 1820, after having filled, in the capacity of assistant, the chair of Dugald Stewart, from which this illustrious professor developed the principles of moral philosophy. Sir William Hamilton was among the candidates, but was unsuccessful, notwithstanding the suffrage of Dugald Stewart himself, who had rendered homage to his rising merits. It was not till 1836, in consequence of the retirement of Dr Ritchie, that Sir William Hamilton, now properly appreciated, obtained the vacant chair of logic and metaphysics. It was honourable for France to witness, at this period, one of our professors, M. Cousin, supporting Sir William Hamilton's claims with his influence. Success crowned his wishes; M. Cousin had no small influence in the nomination of the Scottish savant, and he deserves the praise of discovering the merit of a stranger whom his fellow-citizens had not always judged of with the favour he deserved.

Among the remarkable circumstances in Sir William Hamilton's literary life, may be mentioned the discussion between him and the partisans of the phrenological doctrine, of which the principal representative was Dr Spurzheim. The occasion of it was two memoirs written by Sir William Hamilton in 1826–1827, ***On the Practical Consequences of Dr Gall's Theory of the Functions of the Brain.*** These memoirs, and such as appeared in the English reviews, of which we have formerly spoken, compose all the literary works which Sir William Hamilton has published; but of what importance is the quantity of his works? is it not from their effect solely that the public ought definitively to form a judgment?

The general character of this author's thought is that which marks the spirit of the whole Scottish philosophy; the examination of the fundamental point of metaphysical science. Now, what is this fundamental ontological point? It is the very possibility of philosophy, the determination of its object and its domain. The Scottish school has defined philosophy to be, the natural history of the human mind. According to this definition, all that is beyond the reach of observation, is by that very circumstance without the limits of the science. Sir William Hamilton has illustrated and developed this idea; he has explained the doctrine of *common sense.* He has skilfully taken up a position between scepticism and dogmatism, and, drawing from the principles of the school of Kant, he has combined them with those of Reid and Dugald Stewart. He has perceived how to avoid the rock on which the Scottish philosophy has struck, the want of a logical tie and connection in the explanation of facts. It is the absence of this systematic method which has subjected this school to the reproach of eluding questions instead of answering them—of suppressing difficulties rather than solving them. Restoring dialectic to its true place, he has replaced it in the rank it ought of right to occupy at the head of the sciences. The richest erudition in all matters of philosophy likewise distinguishes Sir William Hamilton's works; versed in the study of the German philosophy, he has not neglected antiquity, the primary source of all our researches and of our means of comparison. Mr Brandis, a professor of high reputation in Germany, has called him the *great master of peripatetism.* Finally, Sir William Hamilton, while preserving all the philosophical character of his nation, and losing none of his originality, has been enabled to unite therewith all the benefits that flow from an enlightened criticism, and the examination of the principal scientific results among foreign nations.

These preliminary considerations, useful when we are about to enter upon the examination of a work so important as the present, are preceded, in the translator's volume, by some general views of the character of philosophy in France in the nineteenth century, of which we shall give a rapid exposition.

According to M. Peisse, the principal schools may be summed up as the following:—the ***Sensualist*** school, the ***Spiritualist,*** the ***Scotch, German,*** the ***Progressive*** (*celle du progrès*), and another, which combines the attributes of ***Scepticism*** and ***Mysticism.*** In his opinion, the first mentioned of these is the most numerous, the most popular, and the most national. Sensualism prevails among all the learned professions, medicine, the natural sciences, and even in political economy. But, banished from the

Sorbonne, it has particularly established itself in medicine; it has there created a new category of applications which, under the name of Phrenology, has brought together a pretty considerable number of disciples. The Spiritualist school, the leading members of which are of considerable influence, is divided into two branches, the Scotch and German philosophy. The first was introduced into France, almost suddenly, after the prelections of M. Royer-Collard (1811 to 1813); afterwards supported by M. Cousin, then by M. Jouffroy, it has brought into France a method founded on experience, having for its object the empirical science of the human mind, facts for its basis, and Bacon and Newton for its masters. It is exclusively scientific, and consequently gives offence to no received opinions, which is perhaps the cause of its reception having been so prompt and easy. Certain points of relation likewise unite it to the sensualist philosophy, and it has contracted an alliance with this school, which may have promoted its popularity.'

But the same motives to union did not exist between the Scotch and German school, nor, consequently, between the German school and the French mind of the nineteenth century; accordingly, the influence of Germany has been less considerable than that of Scotland. At no period, moreover, has France much relished the German spirit: Leibnitz, who wrote a part of his works in French, established no school in France, while his cotemporary Locke had little difficulty in making an impression on the mind of the masses. The reason of this is, that the French character is more curious to know than desirous of assimilating foreign elements; better calculated to judge of than to appropriate to itself the riches of others. The German philosophy has, nevertheless, taken root among us by means of some works of detail; numerous works have been translated, and certain professors, among whom M. Cousin may be mentioned, have adopted a portion of its principles and methods, subjecting them at the same time to considerable modifications.

The Spiritualist school is the one which, at this moment, can boast of the greatest number of adepts: represented by professors of no small popularity, it has obtained the support of public opinion. M. Peisse does not, however, predict for it a very long futurity. He believes it destined to prevail exclusively within the circle of the official schools. He does not think that it possesses sufficient vitality to exercise a continued influence over the mind of the masses, and he accuses it particularly of a false enthusiasm, and a natural inclination to mysticism and obscurity. The school called the Theological, created by a spirit of reaction, does not appear to him to possess in any higher degree the necessary means of long duration; but he places more confidence in the elements which constitute the doctrine which people have agreed to call the *Doctrine of Progress;* a kind of ramification of St Simonism, but which has the merit of extending the field of science, by directing it towards the perfecting of the whole of humanity. We may here use M. Peisse's own words, as he justly characterizes the influence of this new philosophy, by comparing it to the known influence of many other systems which have existed in history:—

"We shall now make one concluding observation. This school (that of progress), placing its point of departure in the social action, is evidently on the fair way to success and popularity: it rests on the most active interest of our times—the political. At no epoch, in fact, has philosophy (whatever definition may have elsewhere been given to what bears this name), enjoyed any celebrity, splendour, or power, but by its alliances. In the times of antiquity it never emerged from the schools till it began to interfere, by its practical action, with public and private morals, in the forms of Epicurism, Stoicism, and Mysticism. In the middle ages it had no influence on the public mind but through the channels of theology and religion. After the Cartesian reform it identified itself with the scientific movement, and was there almost entirely absorbed. The philosophers of these times were Copernicus, Descartes, Leibnitz, Newton, Galileo, Bacon, Gassendi, Huygens; to these may be added the Academy of Sciences of Paris, and the Royal Society of London. In the eighteenth century, philosophy introduced itself by every possible way into the political order; it is the sign, the name, the standard, and the lever of the revolutionary movement, in the midst of which we still live. Its three great philosophers are expounders of public law; one writes the Essay on the Genius and Manners of Nations; the other the Spirit of the Laws; and the last, the Social Contract. Then come Turgot, Condorcet, that is to say, the Economists and the Constituents. The Theological school also mingles with the spirit of the times, but it is by way of reaction; it is of no influence but by resisting. The Eclectic school abandoned its active part too early and completely, by refusing or neglecting to resolve the social questions, and thereby compromised not only its influence but even its existence. The St Simonian school, on the contrary, and all its off-shoots, Fourierism, and its connections, again took up (under forms, and by means, which it is useless to attempt to appreciate) the inheritance of the preceding age. Thus, through all, and even in spite of all their deviations, absurdities, and even follies, these sects have struck deep roots; they have warmed the imaginations, modified the spirit of economical and political science, filled the minds of statesmen and governments; they have given a colour to general literature, and even introduced into language new words which have almost ceased to be barbarous.

"Up to the present time, in truth, all these doctrines have been rather borne up by the spirit of the times than supported by their philosophical value; they have found no representatives but in minds less original than eccentric, and have been most frequently produced under the extra-scientific forms of mysticism and illuminism. In a literary point of view, they have given birth only to works void of taste, infected with neologism, and in which a false originality is an unequivocal symptom of want of power. In general, the resources of mind, erudition, reasoning, and talent in the writers of this school are far from being in conformity with the gigantic proportions of their undertaking." (Preface, p. lx.-lxiii.)

M. Peisse's conclusions regarding the present state of philosophy in France are, that these different schools appear destined to be mutually tolerant of each other; they live in peace, or rather in a state of mutual indifference.

"Thus, as I have stated at the commencement, all these schools and doctrines, the existence of which can be discovered by the researches of the critic and the historian, subsist apart from each other; they seem resigned to tolerate and reciprocally admit each other in virtue of the right of legitimate concurrence, just as if a place could be afforded for every one in the region of thought, in the same manner as in the region of space. Each of these schools, retrenched within its own private domains, willingly consents to make no inroad on the territory of another, provided that other exercise the same forbearance towards it. By this piecemeal proceeding, which likewise affects the higher branches of knowledge and art, philosophy abdicates her highest function, which is a mission at once universal, directive, organizing, and legislative. Reduced by these admitted fractional partitions to the restricted proportions of a subordinate study, she loses her high and independent position. Instead of being the connecting principle, the key, and the common centre of all the sciences, insulated from them, and ruling over them all, she permits herself to be absorbed by them, and can claim no object, notion, or fact which they do not dispute with her. As a branch of study co-ordinate with all others, she is far from being in a position to maintain herself even in this equivocal rank, and to advance along with them on a footing of equality; rejected on all sides as a superfetation which represents nothing, and knows not even to what she should affix her name, she will gradually disappear from the scene; for we may truly say of her, reversing the words of the poet, that she obeys if she does not command, *Paret nisi imperat.*

"This tendency to decline betrays itself even materially in the exterior means by which it is intended to be taught and propagated. The few chairs nominally designed for a superior kind of instruction in philosophy, are almost silent, for the masters whose voice was formerly heard there, have retired and left them empty. The official programme of philosophical instruction is otherwise characteristically insufficient, both in regard to the number as well as the nature of the courses. The Faculty of Letters in Paris has only three chairs of philosophy, and two out of these three are devoted to the history of the science; and the only dogmatic chair existing in the capital has been for many years so neglected, that it may be said to be vacant. In the College of France, that great subsidiary to the University, the focus of all the higher studies, philosophy could preserve a place in its extensive programme, which forms a complete encyclopædia, in no other way than by presenting herself as a branch of ancient literature and philology. Finally, there do not exist throughout all the rest of France more than five public courses of philosophy in the five Faculties of Letters. There is not a German university which does not offer almost as many advantages, in this respect, as the whole kingdom. Does the teaching of private individuals offer compensation? If we examine, we shall find that it affords none, absolutely none. Apart from the means of teaching it, we find the same spectacle. Philosophy has no avowed organ in the immense machinery of the periodical press, and this is a fact of the most significant description. Its only public asylum is the Academy of Moral and Political Sciences, where it is, thank God, very worthily represented, but even there it had difficulty in obtaining a portion of the attention and interest which were disputed with it by statistics and political economy. Books still remain, which, by their abundance, may give rise to some illusion, and belie the picture given above; but it must not be forgotten, as I have already remarked, that the great majority of these publications belong to erudition, philology, history, criticism, in a word, to general literature rather than to philosophy." Preface, p. lxv—lxviii.

It is by this interesting discussion, conducted with skill and sagacity, as well as a careful observance of facts, that M. Peisse introduces us, by a natural transition, to the examination of the following fragments, which will afford us a term of comparison between the works of France and those of other countries, and enable us to judge of the character of the metaphysical sciences in Scotland. We shall ourselves select from these fragments what is most new and original.

The first of them, entitled, *Cousin-Schelling*, is an examination of M. Cousin's system of philosophy, in its relations with the German philosophy, and in particular with that of Professor Schelling. This article was written on the occasion of the opening of M. Cousin's course in 1829. Sir William Hamilton endeavours to seize the prominent points in the Professor's prelections; he attributes to him in part the introduction of the rational philosophy into France, and tries to demonstrate in what these doctrines, viewed as a whole, consist.

Going back to the state in which philosophy existed in France at the beginning of the century, he indicates at what point M. Cousin took it up, and in the midst of what influences he announced his own ideas, and endeavoured to construct a new rationalism which, making conscience its starting point, derives from conscience, as interrogated by reason, the whole of the scientific edifice. He scrupulously analyses the Professor's doctrine; we shall briefly refer to it here for the sake of those who may have lost sight of the characteristic features of his doctrine.

Three elements are found in intelligence, which reciprocally presuppose each other, all of them essential and inseparable from each other. These elements or principles, recognised by Aristotle and Kant, are *the infinite* or *unconditional*, the finite or conditional; finally, the relation of the finite to the infinite, which forms the integral element of intelligence.

Reason, in which these three principles appear, is not personal nor individual, it is absolute and divine; it is the true manifestation of God in man. The ideas of which we are conscious, place us in immediate relation with God, and which affords us a means of knowing him; thus God may be conceived of by us, the relation of God to the universe may be manifested to our intelligence. God, the absolute and independent cause of all that exists, may, and must, create; creation thus becomes necessary, and affords to our eyes the striking proof of the existence and action of the Divinity. These ontological principles are likewise those which govern the moral and material world. Every where these two elements again appear,—the finite, the infinite, and their common relation which forms the third element. In psychology, the essence and point of departure of every science, human and divine, we likewise meet with three terms of the same phenomenon: 1*st*, The idea of me and of not me as finite; 2*d*, The idea of some other thing, as infinite; 3*d*, The idea of the relation of the finite to the infinite element. What constitutes psychological science, likewise constitutes the science of the history of philosophy itself, for the latter is just the history of human reason, with all its relations, its laws, and vicissitudes. Four systems or partial views of human intelligence divide history and include all opinions; these systems are, *Sensualism, Idealism, Scepticism,* and *Mysticism.* None of them is false, but in as far as it is incomplete; thus, all are true, inasmuch as they affirm, and false, inasmuch as they deny; the electism founded by M. Cousin should reconcile them, and bring together the portion of truth which each presents, without having the power of itself to shew it entire.

Sir William Hamiltón has illustrated and discussed what we have here reduced to a mere skeleton, but the subject has been so often noticed and commented on by the journals of the time, that this will be sufficient to recall it to the mind of every reader in any degree familiar with the progress of philosophical ideas in our times. Sir William Hamilton reviews the most celebrated professed opinions on the subject of the theory of the *infinite,* as the immediate object of knowledge and thought. These opinions, according to him, are reducible to four: that of the author, that of Kant, that of Schelling, and that of Cousin. The Scotch Professor compares them, and makes use of this comparison to remove the faults and imperfections of those in which he does not concur. He makes an attack, chiefly in reference to M. Cousin, on the definition of the absolute by absolute cause, undertakes to demonstrate the falsity of his rational theology, and combats, in particular, his theory of liberty. According to the whole of his observations, he considers it impossible to realize the attempt of establishing a general harmony among all the systems; but, rendering justice to the talents of the author, he pardons him for the bold and vigorous attempt, common to all men devoted to the cultivation of thought, and who, wishing to overpass the limits of our intelligence, would attempt, by a sudden bound, honourable to human nature, to attain even to the knowledge of the infinite.

In a second fragment, still more curious to us, inasmuch as it transports us into a less known field of the Scottish philosophy, Sir William Hamilton institutes a comparison between two celebrated metaphysicians, Reid and Brown. Reid, as may easily be seen, obtains all his sympathies; but this does not prevent him, at the same time, judging of Brown with that impartiality becoming a philosopher and a man of letters; but Reid's philosophy had been combated by Brown; and Sir William Hamilton takes this opportunity of resenting some unjust attacks, which would have been calculated, without his efforts to establish the truth, to lessen, at least for a time, the merit of the founder of Scotch metaphysics, and diminish the number of his followers.

In order to understand this discussion, it must be remembered that Reid is the founder of a system of philosophy which rests on the observation of the acts of conscience; and, by interpreting it better, endeavours radically to destroy the scepticism of Hume. The foundation of Reid's doctrine, and what constitutes his glory, is his *new theory of perception*, by means of which we are enabled to conceive and analyse the foundations of our belief in the existence of exterior objects. According to him, the act of perception is a pure belief, independent of all demonstration, and instinctively determined by the natural constitution of the human mind.

While Sir William Hamilton assigns to Reid's doctrine the advantage over that of Brown, he discovers several errors in the former. He blames Reid for having classed consciousness among the other intellectual faculties, while all philosophers, Aristotle, Descartes, Locke, have considered consciousness, not as a particular faculty, but as the condition itself of intelligence. Sir William Hamilton finds fault with this distinction as neither very logical nor natural, and he forcibly exposes the defects in the analysis of this philosopher, who limits the sphere of consciousness by assigning to it only the knowledge of intellectual operations to the exclusion of their objects. Reid affirms that we are conscious of an act of knowledge without being conscious of its object. Sir William Hamilton opposes this assertion of the Scotch philosopher, because, after having himself interpreted the part performed by consciousness in the phenomenon of perception, he reduces the number of the different systems of philosophy, which this interpretation can furnish, to six, and ranks the opinion of Brown, Reid's opponent, in the latter of these systems. In this system one may conceive the object of perception as a simple modification of the perceiving subject; the consequence which naturally flows from this is the negation of the external world; and it is against this consequence that the author of the system defends himself by endeavouring to establish the reality of external things by various hypotheses. This system may be reduced to the following formula:—*The mind has no consciousness nor immediate knowledge of anything beyond its subjective states.* In order to enable us to judge accurately of this system, Sir William Hamilton compares it with all those which the history of philosophy has handed down to us. He judges of it in relation to the opinions of Descartes, Locke, Malebranche, and Leibnitz; and, with this vigorous analysis before us, it is not difficult to allow ourselves to be drawn over to the opinion of Reid, much more popular in France than that of Brown, but of which a more accurate estimate will be formed by an acquaintance with this curious discussion, one which has been so often renewed in the field of the history of philosophy.

It will be seen that Sir William Hamilton, although a disciple of Reid, can judge of him with impartiality; that he can divest himself of all the influence of sect; and that, while he assigns in this analysis the preference to Reid's system, he does not believe it to be free from important defects; accordingly, the treatise in question is rather intended to refute Brown than to exalt Reid. We have seen with pleasure some pieces of the former of these writers collected at the end of this article under the form of extracts from his lectures. These extracts form so many vouchers calculated to throw light on the discussion.

The fragment on Logic, which follows that on Reid and Brown, is but of accessory interest, notwithstanding the importance of the subject. The author undertakes the task of passing in review the most remarkable works published in England of late years on the teaching of this science. It is a minute critical detail, which only makes us acquainted with the names of some of the professors in the University of Oxford. We here learn that, according to Sir William Hamilton's testimony, the study of logic has been singularly neglected in the universities of Great Britain. These criticisms are preceded by some general considerations on logic and its importance in the study of philosophy, which divest this treatise of any thing of a technical character which might otherwise have belonged to it.

But the best fragment we have noticed in the volume is that in which the author treats of the study of Mathematics. The field which this question opens up is sufficiently vast to merit a serious attention; our author has accordingly devoted to it nearly a hundred pages in this memoir, where the subject is thoroughly discussed. This treatise was written on occasion of the publication of a work entitled *Thoughts on the Study of Mathematics as part of a Liberal Education,* by the Rev. William Whewell; Cambridge, 1835.

Do mathematics favour the superior development of the mind? Do they form it by enlarging its faculties? Such is the question treated of in this Memoir and answered in the negative. Adducing the testimony of a great number of authors, and the support of numerous examples, Sir William Hamilton undertakes to prove, in opposition to the authority of the Cambridge professor, that mathematics do not afford a general education to the mind. This opinion, which is maintained by modern German professors of celebrity, is likewise that of Voltaire and Franklin, both of whom had cultivated this science. It will probably excite surprise to see the authority of Descartes himself likewise turned against mathematics, a science which he had cultivated with so much success; this is shewn by a fragment of his life by Baillet, quoted in this volume, and in which the French philosopher acknowledges that his own experience had convinced him of the small utility of mathematics, especially when cultivated on their own account, and without applying the means which they afford us to the acquisition of other kinds of knowledge. Sir William Hamilton then compares philosophy with mathematics, and examines the aids which they respectively afford to the intellect. Claiming the whole preference for philosophy, he affirms that a too exclusive study of mathematics renders the mind incapable of observation, whether internal or external, of abstraction and of reasoning; to these disadvantages he adds that of precipitating the mind either into a state of blind credulity, or of irrational scepticism.

But, again, if the study of the mathematical sciences cannot, like logic, fortify the reason against the errors of thought, may it not at least strengthen the reason itself? Sir William Hamilton does not think that it can. According to him, the principles of mathematics being self-evident, every step which the mind takes in the process has the same degree of evidence; every step in a mathematical demonstration can be easily made, and requires only an easy application of thought; and as a faculty is always developed in proportion to its degree of exercise, it thence follows, according to him, that the mathematics, by submitting the intellectual powers to a very feeble degree of activity, develope them in a very limited manner. Further, relying on the opinions of different writers of distinguished character, he undertakes to shew that the study of mathematics is accessible to all, and requires no special adaptation. The testimonies cited are those of Berkeley, S'Gravesande, D'Alembert, Gibbon, Mme. de Staël, and others, who, although less celebrated, nevertheless lend their authority to countenance this conclusion. He exposes the double tendency to credulity and scepticism, which often leads the individual astray who gives himself up exclusively to sciences of calculation. We cannot help thinking that there is somewhat of exaggeration in this assertion, which is very like a paradox skilfully defended; but it is pleasant to follow the animated pen of a writer fully master of his subject, while he draws deductions always well connected, and supported by an accurate acquaintance with the history and minute analysis of human intelligence.

Sir William Hamilton concludes by blaming the University of Cambridge for giving too much encouragement to the study of mathematics in preference to the other sciences. Resting his views on the principles already explained, he points out the impropriety of directing the minds of youth to this in preference to every other kind of instruction, seeing that it is of importance to fortify the intellect with resources adapted to be useful in every circumstance of life, and not in some one in particular.

Such is the volume of Fragments we owe to the Scottish Professor.

Every one will peruse with interest this collection of four dissertations, all of which throw light on the questions of which they treat, and indicate a rare power of analysis, and very uncommon sagacity. We should be glad to see many similar pieces on the moral sciences adorn the pages of our periodical reviews; such memoirs, without pretension or borrowed splendour, afford real instruction, and familiarize the reader with all the questions of the science. Thus reduced to less extended proportions than in a long and elaborate work, the science becomes simplified under a skilful pen, without contracting anything narrow or mean.*

Notices of Earthquake-Shocks felt in Great Britain, and especially in Scotland, with inferences suggested by these notices as to the causes of the Shocks. By David Milne, Esq., F.R.S.E., M.W.S., F.G.S., &c. Communicated by the Author.

(Continued from Vol. XXXIII. page 372.)

At Alford Manse, Aberdeenshire, about eighty miles N. E. of Comrie, "the earthquake was felt at half-past 10 P.M.; but owing to the great alarm occasioned in the family, there may be an error of some minutes. At the moment of the shock, I was sitting reading at a table, with candles before me, nearly in the middle of the dining-room, with my back directly to the south-west, and face to the north-east. Suddenly I heard a loud noise behind, and also under my feet, and immediately felt my chair raised up, and inclined forward at a considerable angle under me; and as I was catching the table with my hands to save myself from what I conceived to be an impending fall, the motion of the chair was as suddenly reversed, and feeling as if I were in danger of being thrown backwards, I clung to the table, which I had just seized, to escape a backward fall,—but the chair directly settled into its horizontal position without any farther oscillation. As the noise continued, I became instantly convinced that I had felt an earthquake, and any danger from it seeming over, I sat still with the view of analysing, at the moment, all the sensations I had experienced, and estimating the character and duration of the noise. I became aware on reflection, and when my attention was no longer arrested by the imminent danger of falling, that the table before me had sustained a vibration similar to that of the chair on which I sat. The south-west side of the table had become elevated above the level, and again immediately became depressed below it. I became particularly sensible of the depression of the south-west, having been impressed with the fear that the candles would be thrown down upon me, but the extent of the movement was not such as to make the candlesticks *totter.* I could make no doubt that the whole house had undergone a similar vibration to those of the chair and table of which I was so sensible,—or rather that the vibration of the house comprehended within it those of the chair and table.

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* From Bibliothèque Universelle de Genève, No. 80; Sept. 1842, p. 210-225

"The noise was of two distinct kinds. The front of the house is about directly southwest, and the first noise heard, was as if an immense quantity of small but sharp shingle had been tilted against the foundation of the front wall, and poured inward below the whole house. The shock instantly followed, and was accompanied by a creaking and rattling of the doors, windows, and various articles of furniture, amidst which a sharp rattling of the slates on the roof was distinctly sensible. This latter noise was not of a continuous and uniform kind, and did not last long—not longer, I think, than about a second; but that which resembled the grinding noise of tilted shingle, extended itself, apparently under ground, on all sides, and became an immense volume of sound, gradually, however, diminishing in intensity, and dying away first in the southwest, and finally in the north-east, after an interval of four or five seconds from its being first heard.

"About a quarter of an hour previous to the shock, Mrs Farquharson had gone into the nursery on the same flat with me, which is that above the ground story; and a young lady then in the house had retired to her bed-room on the same flat, while my eldest daughter had retired to hers in the flat just above me. I had scarcely estimated the duration of the noise, when Mrs F. suddenly entered the room where I was sitting, and stated that the young lady on the same flat had risen from her bed, and come to her in great alarm, saying, that she had certainly experienced an earthquake. At the same moment, my daughter descended from the upper storey, saying that there was some person in her room, who, after shaking her bed, made several heavy steps across the floor, and had at last fallen down in it. I felt it right at the time to calm these alarms, without acknowledging that there had been any earthquake. In the morning, I learnt from the young lady in the lower flat, that while in bed, which stands lengthwise south-east and north-west, she had felt herself, by the rising of the west side of the bed, suddenly tossed towards the east, and as suddenly again thrown down towards the west. She described the noises she heard at the same time, in a way similar to that in which I have done above. Mrs F. was actively engaged at the moment of the shock, which she felt, and she also heard the noise, but imagined it was a violent gust of wind, of which there had been several in the previous part of the evening.

"The house stands upon a bed of shingle, anciently deposited by the small river Leochal. The rocks, only slightly covered, over all this neighbourhood, are micaceous schist and granite."

(5.) *Accounts from Districts East of Comrie.*

Near *Kinross*, at Shanwell, the residence of the Rev. Mr Coventry, the shock is thus described by him :—"At the time of the shock I was sitting. A noise preceded it as of a rushing wind, though the air was perfectly still at the time, and this was accompanied by a noise as if of cattle or horses running rapidly past the windows. The duration of the shock was of such a length, as to give Mrs C. and those who felt it, time to speak of it as an earthquake, and to express their feelings in regard to it. She thinks it lasted a minute. The rushing noise seemed to be in the air, as well as the sound like the trampling of horses or cattle. But besides these, and following them, there was heard a rumbling noise as if of carts on a pavement, but more hollow in the sound; and this latter sound was in the earth, and began distinctly on the north-west end of the house, and proceeded gradually to the south-east side, when it gradually died away. The rushing sound in the air was heard both on the north and south sides of the house, the concussion appeared to follow the same direction as the rumbling sound in the earth. With regard to the effects of the shock, Mrs C. felt the floor of the drawing-room to rock and the window to shake; and, in one of the bedrooms, where two of my daughters and a servant were, the floor was felt to be so unsteady, that they were fain to cling to the chimney-piece, and the doors of the wardrobes and the joists of the roof were heard to creak. The inmates of this room complained of being giddy and sick at the time of its occurrence. No observations were made, as to any walls being cracked. The weather was very wet, the barometer high, and the night extremely dark and perfectly still. I understand that at the Old Manse, our friend David Syme's residence, at Kinross, the shock was very violent, and four distinct rockings were felt.

In the town of *Kinross*, the shock was felt very distinctly by most of the inhabitants, and is thus described by Mr Syme, the sheriff-substitute of that county:—"I was sitting alone in a room on the ground-floor in the south-west corner of our house which fronts the south, when, a few minutes after ten P.M., my attention was attracted by a strange hoarse rushing sound in the south. I laid down my book to listen, and almost immediately heard a louder sound, as if of a heavy body falling gently on the floor of the room above, directly overhead, and continuing to roll along towards the other end—the apparent motion being thus from south to north. I was not sensible of any shock or concussion, and did not think of an earthquake, but was startled by the strangeness of the noise, and ran up stairs to inquire, and found that Mrs S., her mother, and two female servants who happened to be in the drawing-room—a very small room on the second floor in the south-east angle of the house (with one window to the south and one to the east), had the instant before felt the shock of an earthquake most alarmingly. They *heard* and *saw* the crystal and china-ornaments on the chimney-piece in motion, and Mrs S. felt four distinct rockings. She thought that the *east* wall was coming *to her;* and her mother, who was a little farther off, that it was going *from her*, and all were sensible of a strong undulatory motion. They think it began at the east side, and that the east wall or gable-end was most affected, but there was no rent of the wall, nor have I heard of anything of the kind in this neighbourhood. A second shock was experienced about two o'clock next morning (24th), by some of our neighbours, but not by us: though about an hour and a half after the first, I fancied I heard the same rushing sound as before, but less distinctly.

At *Perth*, as the author was informed by several of the inhabitants, the furniture in their houses was shaken, and lamps hanging from the ceilings of their rooms, were made to vibrate. On the side of the Tay, opposite to Perth, a crack was formed during the night of the 23d October, on the side of the turnpike-road, where it runs above a steep bank. This crack was noticed early in the morning of the 24th October, and was such as to endanger the integrity of the road. Two days afterwards, a slice of the road along the line of the crack, for about twenty-five yards in length, slipped down the bank altogether.

From *St Andrews*, in the East of Fife, two accounts were received.

Dr Govan of the E.I.C.S. writes,—" I had just gone to bed, which was placed, as nearly as I can estimate, N. by W., and S. by E., when I experienced a smart and sudden movement from below upwards, and as I thought nearly at right angles to the line in which I lay, coming from the S. and W. I immediately said, it was a very smart shock of an earthquake, and looked at my watch, which shewed 10ʰ. 24′ P.M. An undulating movement immediately succeeding, the smart shock was perceived by those in the room, which caused a degree of giddiness. I immediately went to observe the barometer, which stood unaffected at about 30 inches; without, all was quiet and more still than usual.

Dr Mudie of St Andrews writes,—" Colonel Playfair of the E.I.C.S. was sitting with his family on the night of the 23d October. They all distinctly felt the earthquake, and as both the Colonel and Mrs P. had repeatedly felt earthquakes in India, they instantly recognised the nature of the shock. To all of the company, there was the sensation of the earth rising suddenly up, and vibrating before it returned to its former site. The vibration proceeded from the south-west to the north-east, and the gas lamp suspended in the middle of the room indicated by its oscillation a movement in that direction. The Colonel instantly pulled out his watch, and found the time exactly twenty minutes past ten; and whilst he was looking at his watch, he distinctly felt a second shock, not so strong as the first, but the vibration was in the same direction.

"Mrs General Farquharson was in bed at the time of the shock, and she felt as if a person was under the bed, and lifted it up; the ewer in the basin gingled with the motion, and when she rung for her servant, she came in great alarm, thinking, from the rattling of the windows, that some person was attempting to break into the house.

"A young man, a student in a lodging-house, was awakened by the lifting of his bed;—and thinking it was a trick by one of his companions, got out of bed, and seizing a golf-club, continued to strike at the supposed intruder under the bed.

(6.) *Accounts from districts South-East of Comrie.*

In East-Lothian, near *North-Berwick*, as Mr Scougall at Balgone wrote, "the noise or sound preceded the shock. The shock was not tremulous, but undulating. Those who were in bed describe it thus: They felt, as if their beds had been swung from the top. The shock lasted about two or three seconds.

"Dr Moir of *Musselburgh* writes,—"I was sitting in the dining-room of Loretto with Mr Langhorne; but although there is a gas-chandelier suspended from the centre of the roof, which readily vibrates in treading across the room, neither of us were attracted by this or any other circumstance. Next morning, however, in making my rounds, I called on Mr Watson of Pinkieburn, who asked me if I had perceived any thing uncommon on the night before. I said, No. He then informed me, that, from ten minutes to a quarter after ten, while seated in his parlour by the table, he distinctly felt his chair move under him; at the lapse of about two seconds another movement was distinctly perceptible, at which time he said to Mrs Watson, who was walking along the floor, 'What is that? Did you observe my chair moving under me?' 'No,' she replied, 'but there is somebody knocking at the outside of the house.' She then rang the bell for the servant, who was ordered to open the front door, but saw nobody. Here there were two distinct shocks, between which the noise continued, something like a rumbling wind, and came from the west.

"During the same forenoon, while at *Prestonpans*, the same question was put to me by Mrs Hislop (sister to Mrs Cadell of Cockenzie), who was at the time confined to bed. While alone in her bedroom, at nearly a quarter after ten on the preceding night, she felt as if something was raising up the bed from the floor, and the sensation was so perfect, that she involuntarily seized hold of the curtains near her, when a second, and then a third repetition, caused her to grasp them more tightly, and exclaim—'Have mercy on us!' These heavings were accompanied by a sound from the south, which caused one of the windows to rattle during the whole time. A thimble, which happened to be lying on the stand of a mirror on the dressing-table, kept rattling, as also an empty jug within the basin of the wash-hand stand. Strange to say, none of the other inmates of the house perceived any thing of this, although Mr Hislop himself was at the time, but not in the same room, only a few yards' distant. The family then retired to bed, but, in about half an hour after, a deep rumbling noise was heard from the west, both by Mrs Hislop, and by Mr Patrick Turnbull, her nephew, who was awoke by it, and listened for some time, thinking that it was some one sent from the distillery, of which he has the charge, to awake him.

"Lady Harriet Suttie has since told me, that she and Sir George were at Newbyth on that evening, and that the tremors and heavings were felt there to a degree, that attracted the attention of every one."

At *Trinity*, near Leith, Lieutenant Forrest, R. N., felt the shock very distinctly in a house 300 yards from the sea beach. He described his sensations in a memorandum which he wrote down next morning. The following is a copy of it. "Last night, about a quarter past ten o'clock, I had been about ten minutes in bed, when I felt the bed tremble severely under me; so much so, that I asked my wife (who had been confined to bed for two days previously) if she was taken worse? my impression being at the moment that *that* was the cause. She answered that she was not trembling, but the noise and shaking, she thought, was caused by the servants shutting in the doors below (my bedroom is on the first floor); the window all this time was rattling as if from a high wind, although it was calm at the time; and the furniture in the room creaked, as if in the cabin of a steamer going over a sea. There was a tin-case with hot water in the bed, which I heard shaken about very distinctly. I observed at the time to Mrs F., that I was convinced it was the shock of an earthquake, and noted the time in my watch. It must have continued nearly a minute, as I had time to sit up in my bed, and make the above remarks during its continuance."

In *Edinburgh*, the following persons have communicated to the author their several perceptions.

Mr Syme, of the Bank of Scotland, when in his house in North Castle Street, felt the shock, and a noise accompanying it. The noise seemed to be above his head, in the upper part of the house. Keys hanging on the key-hole of a book-case were made to dangle.

Mrs Swinton, in Athole Crescent, was in bed, and felt the shock. It appeared to come from the north. Her bed rocked twice or thrice. She has felt several shocks in India, of which only one was more severe than this.

Mr M'Callum, of the Bank of Scotland, when in the fifth storey of the bank (about 120 feet from the ground behind it) felt the shock between $10^h\,5'$ and $10^h\,20'$. He first experienced a tendency to fall over towards the east. He distinctly heard the floor near the east gable shake. One window rattled, facing towards the east.

At *Dunning*, about 16 miles SE. of Comrie, the shock is stated by Dr Martin, physician there, to have been felt about $10^h\,14'$ P.M. "It was a kind of double shock, consisting of two strokes in quick succession, with about half a second between them. The first was much the strongest blow. In about half an hour after, another shock was felt, but weaker, and of shorter duration.

"The first or double shock lasted about 5″; the second about 2″ or 3″.

"As to the nature of the concussion, it seemed as if some subterranean element had suddenly struck the solid surface of the earth from beneath, with such a force as to make it yield a little upward. The tremor that followed, arose from its own elasticity and the violence of the impulse. It was both a tremor or vibration of the earth's surface, and an undulation of the ground. At the commencement of the shock, it was a sudden double jolt and tremor of the earth's surface, the result of a subterraneous blow quickly repeated, and, at the end, an undulation or movement of the ground. Objects were more rocked and shaken by the tremulous motion than by the undulation; but none of them were lifted up and let down again. The surface of the earth and buildings thereon, houses, and furniture therein, were moved simultaneously, and trembled or shook altogether as one continuous integral.

"With regard to the points of the compass, the first inclination was nearly in the direction of the north-west. It was the effect of an invisible sudden force, and was quick. The motion back again was slower, and appeared to be the mere recovery of balance or perpendicularity.

"It seemed to travel with great velocity, and was loudest at its termination.

"The 23d of October 1839 was cloudy, with rain; the hills were foggy; wind east, with calm intervals. Much more rain fell than usual in the autumn of 1839.

"About a mile from Dunning, in a farm-house situated on a high level, and founded on whinstone rock of unknown depth, the concussion so marred the swing or vibration of the pendulum of the clock, that it stood still.

"The mounds of earth covering potato-pits were cracked from end to end, and the water of sundry wells was made drumly."

At *Muckhart,* situated at the opening of a gorge on the south side of Ochils, and about 20 miles S.SE. of Comrie, Mr Harvey heard and felt the shock. He writes,—"Having been at Comrie some years ago, when there was a very smart shock, the moment this of the 23d October commenced, I said to a friend with whom I was conversing at the time, 'An earthquake !'—'It is the same sort of sound (he added) that we heard the other day in the harvest field.' I took observation of the time, and all this passed while yet the sound of it was heard; we concluded that it lasted above 50 seconds. As to the sort of sound, it resembled in its approach a multitude of coal waggons on a railroad somewhat as to sound, but chiefly as to the motion produced; there was a quick vibration. My house stands on a bed of channel. There is another near it on mossy ground, and there the shock was felt as a heave. The inhabitant imagined, being in bed, that some huge animal had got beneath his bed and was bearing up the bed to get from beneath. No walls cracked in this neighbourhood, so far as I can learn, but there were several bursts of earth, and slides on the sides of the hills, and breakings of wellheads. Birds' cages moved like pendulums. Noise accompanied, preceded, and followed the shock. The noise was continuous, with variation of the sounds. The sound was first like the distant sound of carriages on the public road; as it approached it grew deep and hollow from the earth, and passed away like the effect produced by a close body of cavalry in quick march over a common. It was in the earth. The concussions were most felt in the upper parts of houses. Doors upstairs in my house, were thrown open and moved on their hinges. From all I can collect, it appears it was not so much felt in houses on the hill sides, as in the houses along the bottom of the range; the houses on the hills are mostly built on rock, those along the bottom of the hills on gravel or loose soil. We had much rain previously. One night, in the end of September, from 8 in the evening to 8 next morning, as nearly as I could ascertain, there fell about 1 inch of water in thickness on the ground. Besides shooting stars, some nights after I saw the most splendid meteor I ever witnessed. It was passing from the west to the east, and proceeded in a line parallel to the earth's surface."

At Woodcot, near *Dollar*, about 22 miles S.S.E. from Comrie, the shocks and the state of the weather at the time, are thus described by Mr Walker. "The first concussion felt here was at 10^h 10′ P.M. on the 23d of October, the second about half-an-hour afterwards. The noise preceding the first, lasted about four or five seconds; in the second the duration of the noise was shorter, and I felt no shock. The concussion of the first appeared to me to resemble more the slight lurch of a ship under way, struck by a wave and righting immediately again, than any other motion. As far as I can judge from the situation of this house (at the immediate base of one of the Ochils) and the quarter of it whence the sound and concussion came from, I should say that they both came from N.NW., and went in the opposite direction across the room where I was sitting; I was placed in rather a favourable situation for ascertaining this, as I was reading at the time, with my arms leaning upon the table, and both it and the chair upon which I was sitting were thrown first to one side and then to the other, or, to speak more correctly, first towards the S.SE. and then back to where they had been; the noise was very loud. It seemed to me to be very like what would have been occasioned by some one over head dragging some heavy piece of furniture along the floor from one side of the room to the other, the sound gradually increasing and diminishing as it came towards or receded from the position where I was. The weather on the day of the shock, and also the one preceding it, was uncommonly calm, very foggy towards the evening, and the air at that time felt much warmer than the degree of heat indicated by the thermometer would have led one to expect, and I thought (but it may have been fancy) that there was a peculiar odour perceptible. In the year 1824, when I was at Lisbon, I perfectly recollect having remarked the same thing, though, from the difference of latitude, the heat and the closeness of the air was much more oppressive; and I remember well that the inhabitants of that city were much alarmed at the appearance of the weather, the same phenomena having, they said, been observed immediately before the tremendous earthquake in 1755."

In a subsequent letter, Mr Walker adds,—" I did not *perceive* any leaning of the house to the N.NW., after recovering the perpendicular,—though I have no doubt it must have done so, as your explanation appears to me quite consistent in other respects with what I felt at the time. I was not sensible of the house being lifted up. It appeared to me, as if it had been struck by something which caused it to heel suddenly to the S.SE.;—indeed I can compare it to nothing but the motion of a ship, when she gives a slight lee-lurch."

The gardener of the Dollar Academy has given the following graphic account of what he perceived. "My family had retired to bed; I alone sat reading, opposite the fire-place, which is in the east side of the room. The candle was burning on the chimney-piece, with the snuffer-tray beside it. I was startled by an unusual noise towards the NW., like the rolling of many carriages, or the sound of distant thunder. It appeared to die away toward the SE., and struck me as being immediately under or on the surface of the earth,—not over head. I still looked in the direction from whence the sound came, and perceived the bed-curtains agitated. The bed stood in the NW. corner of the room. There was a looking-glass in the window, which looks to the west.—It also was shaken. The chair which I sat on, was moved first toward the SE. several times, the candlestick in the same direction. The snuffer-tray was nearly thrown down. The motion of the earth was decidedly undulatory; and from the circumstance of the bed-curtains and looking-glass being moved first, and my chair being next moved toward the S E., and the candlestick in the same direction, I concluded that the shock was from the NW. to the SE. I was sitting in a position peculiarly favourable for observing it. My feet rested on one side of the grate, and my whole weight was on the chair. My attention was keenly alive at the time. The noise preceding the shock lasted, I think, about 4″; a shorter time] intervened between the noise and the shock, which lasted also about 4″. The strength of the shock throughout appeared to be the same."

At *Tillicoultry*, a considerable village a little farther to the east than Dollar, also situated on the south base of the Ochil range, Mr Thomson, surgeon there, writes, that "Those in Tillicoultry who most distinctly experienced the shock, agree generally in stating, that there was a decided undulatory motion communicated to their houses, whereby they themselves, and objects on the floor, were, or seemed to be, lifted up and let down again, as if they were rocked in a cradle, or tossed in a hammock at sea.

"Two considerable masses of rock, it is believed, were detached from the face of one of the Ochil hills here by the shock of the earthquake, as the shepherd was on the spot where they now lie, on the preceding day, and did not observe them till the morning after the event. One of these is estimated at ten tons weight. A large rent, of 4 or 5 yards long, and about one foot and a half wide at its widest part, was observed, on the succeeding day, running across a potato-heap, whose whole length might be 12 yards by 2 yards wide. All the houses in our village, which are nearly 300, were more or less shaken. The slates upon certain roofs of the higher houses, and the dishes upon the shelves, clattered against each other—several bells rang—articles hanging from the ceiling oscillated—windows shivered—doors moved on their hinges—individuals walking or sitting, were thrown slightly off their centre. Many who were asleep or in bed, started up in stupid amazement. One man says he was pitched from one side of the bed to the other. In the upper flats of houses, the chairs on which individuals were sitting, and the beds on which they were lying, rocked like a cradle, or a boat gently lifted by a wave.

"It seems to be the prevailing opinion of those who were in a recumbent posture, or in bed, that the couch was first moved from the N. or NE., and that the S. or SW. side was then affected. The motion of dishes, and the rattling of slates, was on the north side of the houses chiefly.

"The majority with whom I have spoken on this topic, think that the shock came from N. or NE., and travelled to S. or SW. This was the impression of those who were a-bed, and is perhaps confirmed by the following facts. The masses of projected rock referred to took the direction of the S. from the N. (the face of the hill is steep, and slopes southward). The rent or fissure referred to, ran from NE. to SW. The persons felt moved towards the S. who were in bed.

"In the months of September and October, the aurora borealis, or northern lights, were uncommonly brilliant, and stretched across the zenith southward farther than I have seen them before; they had a curious *fiery* colour."

At *Alva*, as the Rev. Mr Drysdale reports, "I was moved upon my chair from one side to the other. I was within half a foot of a wall-press, the standards and door of which cracked as if breaking. My house is situated within 300 yards of the Ochil range. It faces due south. I was sitting in a room at the west gable. When I heard the noise, I turned my face towards the east, in which direction it seemed to me coming. When it came, as it were, around me, I felt very strange, and as if there was something like a shock of electricity over my body, beginning at the feet and going to the head. Sitting still in this position, after the noise seemed to have passed to the west, I saw the carpet move as it had been a wave of the sea, and as it undulated along to my chair:—then was my chair moved to the west, then to the east."

The Rev. Mr Brown, parochial minister of Alva, who felt the shock in his manse at the foot of the Ochils, says,—"What I first perceived was a loud and very singular noise, which lasted 2″ or 3″. Immediately after, I felt the house shake violently." I may add, "That before *perceiving* the shock, or thinking that an earthquake was approaching, I felt, during the continuance of the noise, as if I had been slightly electrified. A quivering sensation pervaded my whole body from the feet upwards."

From *Alloa*, situated on the Forth, about S. by E. from Comrie, various communications were received, of which a few may be noticed.

One correspondent writes,—"I felt a remarkable sensation come over me at the time of the shock. But whether it was connected with the phenomenon, or merely a sensation produced by the mind, being instantly aware of what the phenomenon was, which was taking place, I could not determine. The leg of a piano in the room distinctly creaked."

Mr Roy writes,—"I was sitting in the dining-room on the ground floor, reading, one of my arms resting on the table, and the other on one of the arms of the chair on which I was sitting, when I suddenly felt a violent shock (as if a very heavy weight had been thrown on an elastic floor), which made the table move as if from under my arm in a southerly direction. I immediately called out, 'What was that?' to some of the family who were in the room and also felt the shock. The shock was accompanied, or rather succeeded, by a rushing or rumbling kind of noise, resembling the sound of a carriage, passing along the road, which continued for a second or two, and appeared to me to proceed as from north to south or south-east,—at this period, I must say I felt a peculiar sensation just as if I had been suddenly exposed to danger; and when this had a little subsided, I went to the kitchen to inquire whether the servants had been up stairs making any noise, and found them all alarmed, having heard the noise and felt the shock without knowing the cause; I therefore concluded it must have been an earthquake.'

"Another correspondent says,—" The first circumstance that attracted my attention was a sudden and violent gust of wind, accompanied with a more than ordinary rushing noise, as from the north-east, against the window. I then felt the shock, and the doors of the wardrobe, before which I was standing, which are rather loose, rattled sharply four or five times, and the noise seemed to pass to the other side or front of the house, and roll heavily, as if under ground, away to the south-west. The shock excited a most peculiar sickish sensation, such as I think I never felt before."

Mr Donald, writer in Alloa, communicated several circumstances of interest.

(1.) The landlord of the Tontine Inn there was, when the shock occurred, standing at the door of his stables, which front the west, and was leaning with his back on the south lintel. He very distinctly heard the noise, which he thought came from the north. He then felt a jerk similar to that felt by a person leaning on a steam-boat when it strikes a quay. He was precipitated forward about a foot. The bells in his house were set a-ringing, and the glasses on his tables and sideboard were put in motion.

(2.) A steam-boat was lashed alongside of a quay, running nearly east and west. The boat was on the north side of the south wall of the quay, and the paddle-box was within two feet of the wall. There was about a foot and a half of water between her keel and the bottom of the river. An engineer and a boy were sitting in the steerage cabin, the former reading. Suddenly the boat gave "a heavy jerk" on the pier. These two persons immediately started on deck, to ascertain the cause. The vessel was then about three or three and a half feet from the pier, the shock having caused her to recoil, and she was then moving back to it again. Just before the collision, the engineer heard a distinct rumbling noise, as if under ground, which seemed to proceed towards the south. The engineer on looking at his watch, found the time to be between 10 and 20 minutes past 10 o'clock. The shock was felt at the same moment, by another vessel in the harbour.

(3.) Close to Alloa Ferry there is a small watch-house, the back wall of which runs parallel with a wall inclosing the glass-house premises. These two walls are about eleven feet high, and are about four inches apart. The watch-house has a sloping roof, and, in order that the rain falling on it may not run down the back wall, there is an edging of lead which projects from the roof, making the distance between it and the glass-house wall only three inches.

The ferryman was, at the time of the shock, sitting in the watch-house, when he was startled by a noise and concussion, produced by something striking against the wall or roof of the house. He supposed, at the moment, that the glass-house people were playing him a trick, by tumbling some heavy body upon the house. This thought, however, was almost immediately dispelled by seeing some articles within the house moved, and in particular the cover of a pot, which was shaken from the spar of a small table on which it was placed. The noise appeared to come from the N. or NW.

On examination of the premises next day, it was found that the leaden gutter or edging on the roof of the watch-house, had been bent upwards by the pressure of the glass-house wall.

The glass-house wall runs in a direction NE. and SW. It is built on the thick deposit of diluvial or alluvial clay, which extends through all the low grounds adjoining the river Forth in this part of its course.

Considering the height and distance from each other of the two walls just described, it is plain, that if one remained stationary and the other leaned over, the deviation of the latter from the perpendicular, must have been at least 1° 18′, in order produce simply a *contact*, but no *pressure* of the walls, at height of 11 feet from the ground. If then, this deviation to be ascribed to a rising of the ground, such as would be used by the propagation of a wave along the earth's surface, he surface must have inclined or sloped to at least the extent f the above angle, so that the wave must have formed with the horizon an angle of more than 1° 18′.

But is it a probable supposition, that one of the walls would remain stationary, whilst the other leaned towards it? If the wave came from the north, the glass-house wall would, no doubt, be first affected; but would not the back wall of the watch-house be also made to lean over almost simultaneously? It is true that the two walls were at the foundations only 4 inches apart; but then the back wall of the watch-house formed one side of a solid building, abutting against two gables 14 feet long. The back wall of the watch-house, therefore, would probably not move until the wave had advanced far enough to affect the whole building. Moreover, it is plain, that the house would not by the supposed wave coming from the north, lean over so much as the glass-house wall. A wall at right angles to the *course* of a wave would deviate from the perpendicular, whilst a wall running in the *direction* of the wave would scarcely rise at one end and be depressed at the other, but would, if at all affected, be rent. In this way it may be understood how the back wall of the watch-house abutting against the north ends of the gables would remain vertical, and would be reached by the upper part of the glass-house wall as it leaned over.

(7.) *Accounts from Districts South of Comrie.*

On the south side of the Forth, and at *Airth*, nearly opposite to Alloa, about nine miles east of Stirling, the concussio was felt as one undulation or heave, with but little noise.

At *Throsk* house, about four miles east of Stirling, situat in the low flat Carse land, only seventeen feet above high-wat mark, effects, in some respects similar, were observed. Jeffrey, "whilst sitting at a writing-desk, was suddenly mo forward by a very heavy undulation, which I immediately cluded to be an earthquake. The undulation came from the north and proceeded to the south; and after it had passed, it was immediately followed by a tremulous movement of the earth from the west towards the east, and then from the east back again to the west. The clock which stood upon the north wall of the house, was several times moved towards the south, and once, I think, was five or six inches off the perpendicular. In the flat immediately above the ground floor, where I was sitting, and in the attic storey, the shock was so severely felt, that some of the members and servants of the family were raised from their sleep; and some of them were nearly thrown out of bed altogether. The shock must therefore have been more violent in the upper than the lower parts of the house.

"The first heavy undulation, which I have already mentioned, appeared to elevate me about four or five inches, and then I gradually sunk down again, precisely in the same way as a boat falls down, after having been lifted upon the top of a high wave. The tremulous motion which followed this, was much more sudden though less violent in its effects. No noise of any kind preceded, accompanied, or followed the earthquake at Throsk. As I immediately went to the door to examine the state of the atmosphere, every thing was perfectly still; there was not a breath of wind, but the rain fell heavily, as it had done (I think) the two preceding days and nights, without intermission. I may mention a circumstance which I have not seen taken notice of in any account which I have seen given of the late earthquake, and it is, that I am confident that it was accompanied with an electric shock. I was perfectly calm and collected at the time when it came on, and never had any doubt of what it was, nor was I at all alarmed for the consequences; but the feeling produced upon my body, was exactly similar to what an electric shock has in other circumstances had upon me. In this opinion I am not singular. The Rev. Mr Brown of Alva is confident that he felt an electric shock likewise; I may also mention that the sound which he heard, was very loud and terrific. I do not think, at least I have never been informed of the sound being heard any where but upon rocky and rising ground. The earthquake did not last more than five seconds altogether. So far as I have been able to collect information, the shock was much more severely felt on the low lands, along the banks of the Forth, than on the rising ground which rests on different strata."

In a subsequent letter to the author, Mr Jeffrey gives some farther particulars. He says that "the first heavy undulation proceeded, as nearly as I have been able to ascertain, from N. by E. or N.NE. to S. by W. or S.SW. No tremulous motion whatever began, until the first undulation passed. I find I have not stated quite accurately, in my previous account, the motion of the clock. It was thrown from the north wall of the house, by which it stood, to the south, and was moved five or six inches off the perpendicular by the first shock. Of course, both clock and wall were off the perpendicular at the same time. But as the clock was not attached to the wall, when it returned back to its original position it seemed to rock and swing for a space, until it recovered from the forward impulse which it had received. It made a considerable noise, as did also the crockery in the room. There were only three undulations, or rather *one undulation* and *two tremulous* motions. The first undulation we have already noticed. The first *tremulous* motion proceeded from the west to the east, at about right angles to the line of movement of the heavy undulation, the direction of which is stated above; then the second tremulous motion proceeded from the east to the west, at about right angles to the same line. Now, these cross tremulous motions, which were partly undulatory, were *concave* and not convex. The site of the house, which was first moved by these slight cross shocks, sunk. The motion was very much like that of a ship, when struck by a heavy sea; she lurches over to the one side, and as she falls down between the two waves, she gradually rights, until the masts become perpendicular, without the side which dipped into the water first being elevated at all; that side does not rise, the other side only comes down to the same level with it. I may likewise state, that these two tremulous motions, at right angles to the path of the first undulation, did not appear to me to be occasioned so much by distinct shocks, as they seemed to be something like the settling down of the earth, after the first undulation had passed. Yet it must be observed, that they were quite distinct and separate from the first undulation. In order that you may obtain the inclination or slope of the wave, which was convex, I have measured the height of the clock, which is exactly 6 feet 11 inches. You may take the distance of the clock off the perpendicular, at 5½ inches. The slope of the table at which I was writing was, as nearly as I can determine after trial, about 7° from the horizontal. I am quite certain that the two tremulous motions were exactly across the path of the first undulatory shock; the former were concave, the latter was convex; the former had not above one-fourth of the power of the latter, though, taken together, they lasted rather longer."

In another letter, Mr Jeffrey corrected the estimate he had made of the degree to which the table was inclined, and limited it to 4½ degrees. He also added, that he has "a sensible recollection of hearing the pendulum strike the sides of the clock,—but how often I cannot say." As to the duration of the undulation and the lateral vibrations respectively, Mr Jeffrey observes,—"The first undulation took 1¼″ to pass,—then say that ¾″ elapsed between the first undulation and the side movements. This leaves about 3″ for them, which is perhaps rather above than below the time which they occupied."

Two things are remarkable in these accounts from Throsk, especially when contrasted with the effects observed in places not remote from it. The *first* is the absence of all noise, notwithstanding the violence of the heave. The *second* is the extent to which the clock and table were seen by Mr Jeffray to deviate from the perpendicular. Judging by the eye, Mr Jeffrey thought the table sloped to the horizon at an angle of about 4½.° On the other hand, if the top of the clock was 5½ inches from the perpendicular, it must have deviated to the extent of only 3° 47′.

In *Stirling*, as the Messrs Drummond wrote, "the shock was most perceptible around the base of the hill, where a combination of the heave and tremor was felt. In the upper parts of the town situated on *rock*, there was merely a tremor or vibration." The rock here referred to is a mass of greenstone, on which the castle and the older part of the town is built.

At the base of this rock, the deposit of carse or finely laminated clay extends on all sides of it, except the south, where there are accumulations of sand and gravel.

At *Thornhill*, about sixteen miles from Comrie, the shock, as felt by the post-master there, is compared to "the heaving of a steam-boat," and as being an undulation. "The bed on which I lay rocked like a cradle, and leaned to the north first and south last. I consider it came from the south and proceeded to the north."

At *Blair-Drummond*, about eighteen miles from Comrie, Mr Home Drummond, M.P., writes :—"The noise was the thing most remarkable. It was usually compared to that caused by a train of carriages passing. It seemed to me to proceed from the north, or a little to the east of north, and to pass off to the south, or a little to the west of south. Had I been in the open air, I could have spoken with more certainty ; but the noise did certainly appear to me, being within the house, to pass in the direction I have mentioned. It continued a good many seconds, perhaps twenty, and died gradually away. Small loose articles were shaken in the houses in this quarter. For two days after the earthquake, we had a constant small rain from the east, without a breath of wind, and the barometer was stationary and very high for such weather. I remember shocks of earthquake which were felt here on two occasions, above thirty-five years ago ; one about 11 P. M. and one about 6 A. M. They resembled more a blow or sudden concussion than this ; but the noise on this occasion was much greater, though I do not think the shaking of the house was much more perceptible now than formerly. The evening was quite still."

From *Bucklyvie*, about twenty-two miles S.SW. from Comrie, a report was received, certified by John M'Ewan, post-master, and Daniel Kennedy, M.D., detailing some of the effects produced on the mossy ground west of Stirling. The report contains the following statements of persons living on the moss, and who go there by the name of moss-farmers :—

"Mrs Napier says noise came off NE. When she heard it, it seemed to be at a distance. In her own words, calls it a long soughing, dundering noise. It died away for some seconds, and was succeeded by a second noise which was louder. She then asked her family, 'Do you think that's thunder;' and with latter sentence in her mouth began to sit down, when, being seated, she felt the earth (in her own words) rowing or coming till me. She sat with her back to the north. One of her sons had his hand placed on jambs of fire-place, when he thought they would fall to him. Her husband was in bed half asleep; being alarmed, got up on his elbows in bed, when, by an undulation, he was thrown down on his back again. From these narrations, it appears to me that the first 'lean over' was to the east. House of these people faces S., and bed lies N. and S., fire-place facing E."

Mrs M'Ewen "was dozing in bed. Heard no noise. She lies E. and W. Her head seemed to lie lower than rest of body, during movement. Old man (*æt.* 80) lies N. and S. His left shoulder received a shock while in a slumbering state, which made him cry to people of house; says it came off E."

"Frederick Campbell says noise was very loud. It came off the south-west or south. After noise, felt motion in bed. Head lies to the south. Felt a pitch to the east first, and then to the west, and there was no more of it. Wood of house cracked. House seemed to be drawn together, and then went back again. House also seemed to sink, just as a ship at sea."

It only remains to be mentioned, that this shock of 23d October 1839 was felt as far south as the English borders. It was felt at *Netherby Hall* by Miss Burdon in a room at the top of one of the turrets; but it was not perceived in any other part of the house.* It was felt at *Closeburn Castle*, Dumfriesshire, at $10^h\ 2'$ P.M.

It was felt at *Selkirk*, and in the neighbourhood also at *Kelso*, where the windows rattled, and crockery ware was shaken. It was felt at *Coldstream*, in the neighbouring village of Newtown and the farm of Mountfair.

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* Letter from J. A. Campbell, Esq., W.S., Edinburgh.

Remarks on Earthquakes in British India, contained in a Letter addressed to David Milne, Esq. by Lieutenant R. Baird Smith, Bengal Engineers, Assistant Superintendent of the Doab Canal, Saharunpore.

Mussoorie, in the Himalayas,
9th September 1842.

My attention was first specially attracted to the subject of earthquake shocks, by the occurrence of that of the 19th of February last, to which many circumstances combined to give to the English in India a peculiar and exciting interest. Its most destructive influence was experienced in the valley of Jellalabad, the chief town of which, of the same name, was at the moment occupied by the small but gallant brigade under Sir Robert Sale, which alone of all the forces in northern Affghanistan, had sustained, without a spot, the honour of our arms and name. They were beleagured by a force at least quadruple their own, which was flushed with recent success, and commanded in person by the most active, energetic, and unscrupulous of the whole of the Affghan chiefs, Akbar Khan; and it was only by labour almost incredible, continued by night and by day, that the miserable defences of Jellalabad had been made even moderately effective. In a moment, the exertions of months were nullified; their bastions, parapets, &c., were thrown open by large breaches; and to the superstitious natives it must have seemed as if their gods had combined with their foes to insure their destruction. But the energies of the "Illustrious Garrison," as Lord Ellenborough most justly styled it, were more than equal to their difficulties; and the final result of their defence is one, which we all contemplate with the pride of soldiers, and feel it to be the redeeming feature of the wretched series of events in Affghanistan, with which you are now familiar.

The details of this earthquake, which was felt from Jellalabad, to Shalkur in Thibet on the north, and to Saharunpore on the south, I collected as they became public, but they proved of a most discrepant and unsatisfactory character. Still I was unwilling to lose so favourable an opportunity for attracting the attention of qualified observers to the subject of earthquakes generally, and I therefore arranged these details and published them in the local journals. The effect more than equalled my anticipations, for a large amount of additional information was furnished me, and I have received from many quarters assurances of active co-operation. Numerous corrections are necessary in my paper on the Jellalabad earthquake, and these it is my intention to make when I prepare the "Register of Indian Earthquakes for the year 1842," materials for which are rapidly accumulating.

Such a subject as the present expands almost insensibly, and I find myself in possession of information that leads me to consider the past as well as the future history of earthquakes in India. From an analysis of details which cannot be given here, I have been enabled to recognise several distinct foci of disturbance so to speak, throughout this country. The classification of these has been limited strictly by the facts in my possession, so that as these extend, modification may be necessary. At present, the following are the most distinctly marked "regions," to borrow a term from Mr Lyell, throughout which the actual foci of disturbance are distributed :—

1. The great central region of the Himalayas, extending from the Burrampooter on the east, to the limits of the Hindoo Khoosh on the west. Undoubted evidence exists of the emanation of earthquake shocks from different points on the *southern* side of the axis of the Himalayas, but none has yet reached me, of any proceeding from the northern, although, throughout the whole of Thibet, indications of igneous action abound. It is, however, probable that such evidence may yet be obtained.

2. The lateral region of the Himalayas. To this belong the earthquakes that proceed from the lateral valleys of the Himalayas, as from the valleys of Jellalabad, of Cashmere, of Katenander, each of which has been the ascertained focus of shocks, which have been strictly local in their effects. Lines of hot springs appear to connect the foci both of the central and lateral Himalayan regions.

3. The region of Sinde and the Delta of the Indus. The country between the Hindoo Khoosh and the ocean is con-

stantly subject to earthquakes, but I have not yet been able to recognise any distinct focus of disturbance throughout the mountains by which it is traversed, and *at present* I consider the shocks as emanating either from the mountains on the north, or from the well known region of Sinde and Cutch on the south.

4. The region of Chittagong and the Delta of the Ganges. Throughout the line joining this with the former region, which stretches completely across the Peninsula, numerous signs of volcanic action occur. The great trap district of central India extends to the right and left of it; hot springs are plentiful, and disruptive action is in numerous instances strikingly evidenced.

5. The region of the Arracan coast. The earthquakes in this region have occasionally been of appalling violence, and the volcanic indications throughout it are of the most interesting and striking character. An archipelago of volcanic islands fringes the main coast, some of which have been active within a recent historical era, and, at this moment, symptoms of activity exist. This region is directly connected with the volcanic train of the Moluccas, and also with the region of Chittagong just adverted to.

6. The ocean region. Relative to this my information is still very imperfect, and I make it distinct in consequence of some very singular phenomena observed in the open sea on the Indian coast, clearly indicating subaqueous volcanic action.

Details connected with each of these regions will be given at a future time,—the arrangement of them is still imperfect, and I would wish you to consider the preceding as a mere outline sketch; but it will suffice to indicate to you the interesting field India presents, and I trust the harvest to be reaped from it will yet prove an abundant one. Our countrymen are distributed over the whole extent of these tracts, and I will spare no efforts to ensure their co-operation. From south India, our information is at present a total blank, but I do not despair of yet seeing it filled up.*

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* We perceive that Lieutenant Baird Smith is publishing the materials he has collected on Indian Earthquakes, in the Journal of the Asiatic Society of Bengal. See p. 242 of No. 123 (39, New Series). 1842.

Remarks on two Points in the Theory of Glaciers. By M. Elie de Beaumont, Member of the Royal Academy of Sciences.*

The lectures which I delivered this year at the Collége de France on erratic phenomena, led me to examine the theory of glaciers, and I now ask the permission of the Society to submit to their attention two theoretical remarks which have occurred to me in the course of my investigations.

1*st Remark; relative to the action which central heat exercises on glaciers.*

The increase of temperature observed in penetrating the solid crust of the earth, gives rise to a constant flow of heat which traverses that crust, and is dissipated at its surface. If we call g the fraction of a degree by which the temperature becomes augmented when we penetrate to the depth of a mètre, and k the conductibility of the terrestrial crust, this flow of heat has as its measure the product $g. k$. This flow of heat would be capable of melting, taking time as unity, a bed of ice whose thickness would be $\frac{g\,k}{75}$. I attempted, some years ago, to calculate approximately this quantity for the surface of the ground at the Observatory of Paris, and I found that the flow of heat which proceeds from the earth, would at that locality melt annually a bed of ice of $0^{m}.0065$ (six millimètres and a half), a result which M. Poisson has inserted in his work, entitled, *Memoire et notes formant un supplément à la théorie mathematique de la chaleur* (Paris 1837.) This quantity may doubtless vary from one point on the surface of the globe to another with the values of k and g; but it seems to me very probable that the variations would not be extensive, and that by admitting that the flow of heat emanating from the terrestrial crust, and dissipated at the surface, is generally capable of melting 6 millimètres and a half of ice in the year, and of producing, by this liquefaction, about 6 millimètres of water, we should not be very far from the truth for any given point.

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* Read to the Philomathic Society of Paris, 30th July 1842. A corrected copy of this paper, and the following one, was kindly transmitted to us by the *Author.—Ed.*

This influx of heat proceeding from the interior of the earth, arrives at the bed of glaciers as at the bed of the sea and of lakes, and, in general, at all points of the rocky crust of the earth. Having reached the bed of a glacier, it conducts itself differently, according to circumstances, as I have already remarked in a note read to the Philomathic Society on the 6th June 1836. (See *L'Institut*, vol. iv. p. 192, No. 162, June 15th, 1836.) The flow of heat may traverse the entire glacier, and then become dissipated at its surface; or it may stop at the bed of the glacier, and be there entirely employed in melting the ice; or, more generally, it may become divided into two portions, of which the one is employed in melting the ice, and the other traverses the ice, and is dissipated at the surface by radiation, by contact with the air, &c.

Hence it results that the *maximum* quantity of water which can result from the action of central heat on the ice and snow distributed over the surface of the earth, is represented by a sheet of water six millimètres in thickness, having the same extent as that ice and that snow, and that the *maximum* quantity which can be produced in *a month*, is represented by a sheet of water *half a millimètre* in thickness, a quantity corresponding with that produced by a very small fall of rain.

The quantity of water resulting from the liquefaction caused by the sun, and by atmospherical actions, is incomparably greater.

In the physical atlas of Berghaus, the quantity of water which falls annually on the elevated portions of the Alps, in the state of rain, hail, or snow, is estimated at thirty-five inches, or 947 millimètres; the snows and the glaciers of the Alps having remained for many ages in a state almost stationary, but more retrograde than progressive, it must necessarily be the case that the quantity of water which flows from them annually (apart from the evaporation) must be equivalent to that which falls in all forms; this quantity ought even to exceed, relatively to the surface really covered by permanent snow or ice, the proportion stated above, because, the slopes which are too rapid for the adherence of snow, throw off all that they receive into the valleys situated at their base, where it accumulates until it liquefies along with that which has fallen directly upon them. It thus appears that we should not exaggerate by calculating at about 1200 millimètres the quantity of water which flows annually from all the snowy surfaces.

Nearly the whole of this quantity must flow off in consequence of superficial liquefaction, and during the six months in the course of which this superficial liquefaction is perceptible, seeing that the six millimètres which can result from the permanent liquefaction beneath only form a very small fraction of it. The quantity of water which the snow and the ice of the Alps give forth during the summer ought thus to amount to 200 millimètres per month, that is to say, about 400 times the *maximum* quantity which the flow of internal heat is capable of melting in the same period.

Hence it results, that in winter, mere threads of water should be seen issuing from glaciers, altogether disproportionate to the torrents which flow during summer; and this, indeed, is the fact, according to old as well as new observations made on glaciers during the winter season; thus observation confirms the deductions afforded by the theory of heat, and is very far from contradicting them, as has been supposed. The quantity of water which the flow of internal heat ought to produce from glaciers in winter, is even so small, that at most it can account for the slender threads of water which are seen running from them; and that the latter may very well represent both the water of liquefaction and the spring water; it is, moreover, quite natural that this small quantity of water should be limpid.

We may nevertheless remark, that however feeble may be the action exercised by the flux of the internal heat on the masses of snow and of ice covering the high mountains, this permanent flow of heat is one of the regulators of the extent of glaciers. If, *the climate remaining the same,* the internal heat sensibly diminished, the glaciers would require to advance into the valleys to a considerable extent, in order that the increase of liquefaction which would take place at their extremity should compensate for the diminished liquefaction produced at the inferior portion of the whole snowy surface.

Any diminution in the influx of internal heat would also have the effect, in the course of time, of giving rise to glaciers at points where they do not exist at the present day; and this is what must take place in remote futurity when the central heat shall have suffered a sensible diminution.

In former times, on the contrary, the flow of heat must have been greater than at present, and this cause must have tended to render glaciers a little shorter; if they were more extensive at a certain epoch, as every thing seems to indicate, such an extension must have resulted from differences between the climate of former periods and that of the present day.*

2d Remark; relative to the influence of external cold on the formation of glaciers.—Certain expressions, perhaps misinterpreted, have been the cause of there being attributed to some of the individuals who are at present occupied with the theory of glaciers, the opinion that the water, formed at their surface during the day, and introduced into the capillary fissures, congeals there during the night by the penetration of the nocturnal cold; but M. de Charpentier, at the end of the interesting work he has published, *Sur les glaciers et sur le terrain erratique du bassin du Rhône*, rejects this idea (p. 307), and even terms it *absurd.* In fact, the conductibility of ice, which indeed has not yet been measured, cannot be very much greater than that of the rocks forming the surface of the ground. It is therefore evident, that the nocturnal cold can only congeal the water in the interior of a glacier to an inconsiderable depth, such as that to which the diurnal variations of temperature penetrate into the ground with a sensible intensity.

But then, how can the water become congealed in the interior of glaciers, as is supposed by the theory which regards their progression as an effect of dilatation? This congelation cannot take place without a considerable abstraction of heat, for we know that water at 0° (32° F.), in order to become converted into ice at 0°, must lose a quantity of heat capable of raising the same quantity of water from 0° to 75° cent. The phenomenon cannot be easily conceived, unless there exist in the interior of the glacier a sort of *magazine of cold:* this magazine of cold cannot be derived from the diurnal variations of temperature; the *annual variations alone are capable of producing it.* During winter, the temperature of the surface of the glacier is lowered to a great many degrees below 0°, and this low temperature penetrates, although with a gradual diminution, into the interior of the mass. The glacier splits up in consequence of the contraction resulting from this cooling. At first the fissures remain empty, and assist in the refrigeration of glaciers by favouring the introduction of the cold external air; but in spring, when the rays of the sun heat the surface of the snow which covers the glacier, they restore it first of all to 0° (32° F.), and then cause the production of water at 0° which falls into the cooled and fissured glacier. This water immediately becomes congealed by the disengagement of the heat which tends to restore the glacier to 0°, and the phenomenon continues until the entire mass of the cooled glacier is restored to the temperature of 0°.

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* I have elsewhere suggested the supposition as to this point, which appears to me the most probable. (See *Annales des Sciences Geologiques.* vol. i., p. 204, and *Comptes Rendus de l' Academie de Sciences*, vol. xiv., p. 101.)

Hence results a certain amount of expansion which may contribute, without any doubt, to the movements of glaciers, but which explains still more distinctly one of the most curious of the glacier phenomena described by observers. It is, in fact, because the glacier *thus augments by intus-susception,* while it melts at the surface, that the stones originally enveloped in its mass are constantly brought to the upper portion, where the superficial liquefaction disengages them, as has been proved during the last year by MM. Martins and Bravais; it is also on this account that the interior of glaciers at last becomes formed of ice nearly pure, as has at all times been remarked by the inhabitants of the Alps.

Even the existence of glaciers formed really of ice, like those of the Alps, thus results from the *annual variations* and not from the *diurnal variations* of the temperature, and it is for this reason that there are no *glaciers*, but only *perpetual snows* under the equator, where there are only diurnal variations of *temperature.*

In proposing this theoretical explanation of the formation of ice in the interior of glaciers, and of the effects that result from it, I by no means seek to dispute the conclusions in the interesting memoir where Mr Hopkins has lately shewn the feebleness of the theory which maintains that the sole cause of the movement of glaciers is to be found in the effects of dilatation. I may even add, in support of the arguments so well developed by the learned Cambridge author, that if the explanation now given be correct, *it is only during a short period* (a few days or a few weeks) that glaciers augment internally, and consequently *dilate each year*. I am also convinced, by many reasons which cannot be explained in this notice, that the phenomena of expansion are not the sole, or even the principal cause, of the movement of glaciers, which, with their numerous *crevasses*, appear to me rather to resemble straps (*lanières*) drawn downwards (as if by the action of a weight), than bars which are compressed and pushed by a force proceeding from above (as would be the case on the supposition of a force resulting from expansion).

On the Slopes of the Upper Limit of the Erratic Zone, and on their Comparison with the Slopes of Glaciers and of River-Courses.

By M. Elie de Beaumont, Member of the Royal Academy of Sciences.*

The interesting investigations of which the erratic phenomena of the Alps have been for some years the object, have contributed to demonstrate an important circumstance that pervades the whole of this class of facts. The traces left by the erratic phenomenon rarely extend to the summits of mountains. They are concentrated in a zone which embraces their base, and which has a well defined upper limit. This upper limit is very frequently marked either by the passage of the rounded rocks (*roches moutonnées*) into the angular rocks, or by the highest terraces formed of erratic materials.

In a district of small extent, the upper limit often seems to be indicated by a horizontal line, but this is an illusion caused by the slight inclination of the line. Although the amount is but little, yet the upper limit of the erratic zone is sensibly inclined; and this limit is formed by a surface which gradually sinks from the centre of the mountainous region towards its edges, cutting the flanks of the mountains in lines very different from horizontal ones.

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* *Read to the Philomathic* Society on the 13th August 1842.

The knowledge of the inclination of the upper limit of the erratic zone, is one of the most essential elements of the problem to which the erratic phenomena have given rise. It is a *bed of Procrustes*, in which all the theories that may be propounded on the subject must necessarily be tested.

We are now in possession of many data as to the absolute height of the upper level of the traces of the erratic phenomena; but these heights have rarely been combined with the horizontal distances of the points to which they refer, so as to admit of the inclination of the surface-limit being deduced. I have made this calculation for the valley of the Rhone from the Grimsel to the Lake of Geneva; for the valley of the Drance from the Saint-Bernard to Martigny; and for the portion of the basin of Lower Switzerland over which the erratic phenomenon of the Valais extends. I have also made it for some parts of the valley of the Aar. Perhaps the publication of these numerical results may cause the publication of analogous calculations for the other valleys of the Alps, and for those of the Pyrenees, the Vosges, &c. The following is the table:—

Height of the Upper Limit of the Erratic Zone.

	Metres.
Near the Col du Grimsel (about),	2300
Near Ærnen (Valais) (Charpentier),	1813
In the basin of Brieg,	1520
In the vicinity of Martigny,	1450
Near the Great Saint-Bernard (about),	2500
At the Mountain of Plan-y-beuf (Charpentier),	1769
Above Monthey,	1157
At the rocks of Mimisse,	1025
At the huts of Playau,	1222
On the slope of Chasseron (Jura),	1050
Geneva (the Lake),	375
Névé of Ober-Aar (limit of the *roches moutonnées*),	2924
Grimsel (the *Col* itself),	2200
Brunig (the *Col*),	1163

By combining these numbers with the distances of the points to which the heights they express refer, measured on Keller's map, I have prepared the following table, which indicates the inclination of the Upper limit of the erratic zone from one point to another.

Inclinations of the Upper Limit of the Erratic Zone.

Points compared.	Distance of the different Points.	Difference of Height of the two Points.	Slope in Decimal Fractions.	Slope in Degrees, Minutes, and Seconds.
	m.	m.		° ′ ″
Grimsel, Ærnen,	25,000	487	0.019480	1 6 57
Ærnen, Brieg,	16,000	293	0.018312	1 2 56
Brieg, Martigny,	80,000	70	0.000875	0 3 1
Great Saint-Bernard, Plan-y-beuf,	15,000	731	0.048730	2 47 24
Plan-y-beuf, Martigny,	18,000	319	0.017722	1 0 55
Martigny, Monthey,	18,000	293	0.016277	0 55 57
Martigny, Mimisse,	44,000	425	0.009659	0 33 12
Mimisse, Geneva,	49,000	585	0.011938	0 41 2
Martigny, Playau,	44,000	228	0.005182	0 17 48
Martigny, Chasseron,	92,000	400	0.004348	0 14 56
Playau, Chasseron,	49,000	172	0.003510	0 12 4
Plan-y-beuf, Chasseron,	110,000	719	0.006536	0 22 28
Great Saint-Bernard, Chasseron,	125,000	1450	0.011600	0 39 52
Grimsel, Martigny,	121,000	850	0.007025	0 24 9
Grimsel, Playau,	165,000	1078	0.006333	0 22 27
Grimsel, Chasseron,	213,000	1250	0.005869	0 20 10
Ærnen, Playau,	140,000	591	0.004221	0 14 3
Névé of the Ober-Aar, Grimsel (Limit of the *roches moutonnées*).	13,500	624	0.046211	2 38 45
Grimsel, Brunig, (The two *Cols* merely, are compared)	29,000	1037	0.035758	2 2 52

This table, if farther extended, would express completely the features of the erratic phenomenon, and would be of utility in interpreting its real nature. We might be guided in the choice of hypotheses by the comparison of this table with other tables expressing similar features in certain natural phenomena.

At the end of my Memoir on Etna,* I have given a table of the slopes of some glaciers. It would be desirable that this table should be extended, in order that we might see what is the lower limit of the slopes on which glaciers are capable of moving. At present I am not acquainted with any glacier in the Alps which moves for a considerable extent (a league for example) over a slope of distinctly less inclination than 3°.

I have also presented to the Philomathic Society a table expressing the features of currents of water, by giving the slopes of the courses of a great number of rivers and torrents. These slopes have, so to speak, neither an inferior limit nor a superior limit, because there are many vertical cascades, and we find the Seine and the Rhone in certain parts of their course flowing over slopes of very slight inclination, of four and of eight seconds. The mobility of the molecules of water accounts sufficiently for the variety presented by the slopes of courses of water. We may remark, however, that the study of courses of water leads us to consider slopes of much smaller inclination generally than those of glaciers: the Rhone, between Lyons and Arles, flows on a mean slope of 0.000553, or of 1′ 54″; the Rhine, between Bâle and Lauterbourg, flows on a mean slope of 0.000647, or of 2′ 13″. Now, the Rhine and the Rhone are two very rapid rivers, and the Doubs, which, in the environs of Besançon, flows on a slope of 0.001000, or of 3′ 26″, reaches about the limit of the slopes of navigable rivers. This slope, however, is only about a fiftieth or sixtieth of the smallest slopes of glaciers over spaces of some extent.

The slopes of the upper limit of the erratic zone are intermediate between those of glaciers and those of the great navigable rivers. They are of an inferior order to the slopes of glaciers, whereas they are of the same order as those of the most impetuous torrents. These slopes, without any exception, would be very considerable for rivers of a few yards in depth, and they would be *enormous* for masses of water having a section equal to those determined by the limits of the erratic zone in the valleys of the Alps, sections having a depth of from 800 to 1000 yards! With such slopes and such sections, the currents of water would have *frightful* rapidity; currents of mud, even the most viscid, forming *nants sauvages* on a gigantic scale, would also acquire enormous rapidity, and be capable of prodigious effects.

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* *Annales des Mines*, 3d Series, vol. x., p. 565 (1836), *and Memoires pour servir à une description Geologique de la France*, vol. iv., p. 215.

The rapidity of a liquid augments with the slope of its surface, and with the depth of its section; of this the rapidity acquired by all rivers when flooded is a demonstrative proof. On the contrary, it is doubtful if a very thick glacier experiences less difficulty than a thinner one in its movement over a gentle slope. This is an essential point to which attention ought to be paid in the comparison of these two classes of transporting agents. Acquired velocity has no share in the movement of glaciers.

Such a difference exists between the *régime* of ice in movement and that of running water, that by preparing *three comparative tables*, one of the above-mentioned features of glaciers, another of those of streams of water, and a third of those of erratic phenomena, a powerful aid would be obtained in determining the cause of the last.

Description of the genus Cuma, and of Two New Genera nearly allied to it. By Henry D. S. Goodsir, Esq. Communicated by the Author.* (No V.) With Plates.

During the summers of 1841 and 1842, I obtained a number of crustaceous animals, which I arranged promiscuously under the genus *Cuma* of M. Edwards, it being my intention to publish them at that time under this arrangement. I waited, however, until it could be satisfactorily proved whether they were perfect animals, or, according to the suspicions of M. Edwards, merely the larvæ of some Decapodous Crustacea. I have now satisfied myself that they are perfect animals, and at the same time have discovered the types of two new genera, which places the group in a still more interesting point of view.

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* Read before the Wernerian Natural History Society, Dec. 10, 1842.

I have applied the name *Bodotria* to one of these genera, and *Alauna* to the other; the former being the ancient name of the Firth of Forth, at the mouth of which all these animals were got; and the latter, the ancient name of the river Forth.

The latter of these genera (*Alauna*) may be the genus *Condylurus* of Latreille, as I have never seen that author's description; but whether it be so or not there cannot be any danger in applying the name *Alauna*, as *Condylurus* had been previously used amongst the Mammalia.

As I had a greater number of specimens of the *Cuma Edwardsii* than of any of the others, I have been enabled to make out the structure of that species with greater minuteness.

These animals are very like small prawns in their general appearance; but they bear perhaps in this respect a greater likeness to the species of the genus *Nebalia* than to any other known Crustaceans.

The shell is hard and brittle, cracking under pressure. All the species are of a pale straw colour. The thoracic portion of the body is large and swollen; it is composed of six segments; the abdomen is longer, and is composed of seven segments.

M. Edwards, in his Memoir on the genus *Cuma*, published in the 13th vol. of the Ann. des Sc. Nat., considers that the whole of the first and largest segment of the body constitutes the head. In all the specimens which I have dissected, I have found a suture running across this segment, immediately before the middle part of it; this is observed very distinctly in the *Cuma trispinosa*, in the *Bodotria arenosa*, and also in the genus *Alauna*. The first of these parts I consider to be the head; the second part as the first thoracic segment. To the first we find attached the rostrum, eyes, antennæ, organs of the mouth, and footjaws four in number. The second part bears the first pair of true ambulatory legs; these legs constituting (according to M. Edwards) the third pair of footjaws.

The second thoracic segment is quite obsolete in M. Edward's species (*Cuma Audouinii*); it is but slightly observed in the *C. Edwardsii*; in the *C. trispinosa*, however, it becomes quite apparent, being of considerable breadth at the dorsal portion. In the *Alauna rostrata* also, we find this segment quite developed throughout its whole extent, and the second pair of thoracic legs arising from it.

These two thoracic segments (the first and second) bear the compound legs in the genera *Cuma* and *Bodotria*, in which two genera the four following segments bear the four pairs of simple legs. In the genus *Alauna*, however, we find a different arrangement, there being an equal number of simple and compound legs, three pairs of each.

The eyes in this tribe of animals are exceedingly small; they are pedunculated, but sessile, and are placed very close together; they are situated near the posterior part of the head, a short distance behind the rostrum, and on the mesial line. They are covered by the shell, owing to which, and their proximity to one another, the animal is at first sight apt to be considered as monoculous. The rostrum is short and truncated in the genus *Cuma;* is almost altogether awanting in *Bodotria*, but is well developed in *Alauna*, being of considerable length and pointed.

The antennæ undergo considerable changes in the different genera of this tribe. In *Cuma* we find the superior antennæ consisting of a single scale-like joint, armed with a number of strong spines; the inferior antennæ* are five-jointed, being in general very little longer than the rostrum. In *Bodotria* the superior antennæ are altogether obsolete, and the inferior antennæ are very short. In *Alauna* again, we find the antennæ more developed; the superior† consisting of a single jointed peduncle, and a long multiarticulate filament which is covered with hairs. The inferior pair‡ are eight or nine-jointed, and are somewhat larger than the rostrum. The organs of the mouth consist of one pair of maxillæ,§ three pairs of mandibles,* and two pairs of foot-jaws.† These last organs will be found minutely described under *Cuma Edwardsii*, the species which I have been enabled to examine most minutely.

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* *Plate II. Fig. 8.* † *Pl. IV. Fig.* 3. ‡ Pl. IV. Fig. 4. § Pl. II. Fig. 2.

The true legs may be classed into compound and simple. The compound legs, as we have already stated, are four in number in the genera *Cuma* and *Bodotria*; but six in *Alauna*. The first, or compound legs, are divided into two parts, the anterior or ambulatory, and the posterior or natatory. The simple legs are much shorter than the compound, and are more adapted for prehension; but they are unarmed with claws, and are seldom used for this purpose.

The abdomen is moniliform, seven jointed, in all the genera. The last joint is very small in the genera *Cuma* and *Bodotria*; but in *Alauna* we find this segment very much developed. All the genera have the sixth abdominal segment armed with a pair of long bifurcated styles. The genera *Cuma* and *Alauna* are quite free of appendages to the other abdominal segments; but in *Bodotria* we find that all the abdominal segments are armed with a pair of bifurcated appendages.‡

Owing to the opacity of the shell, I have not been able as yet to make out the minuter parts of the anatomy of these animals. The intestinal canal consists of a long straight tube, considerably dilated as it passes through the thoracic portion of the body; when it reaches the abdominal portion it suddenly becomes much narrower.

The anal aperture is found in the seventh abdominal segment.

The branchiæ§ are situated on each side of the thorax, immediately above the insertions of the legs, and approach, in their comb-like appearance, to those of the higher Crustacea. Interiorly, each of them is connected with the superior foot-jaws, and excepting that connection, lies apparently quite free in a sac formed by the reflection of a thin transparent membrane, which lines the internal surface of the thorax. The superior part of the branchiæ consists of one continuous piece, which is bent in a hook-like manner at its posterior extremity; the branchiæ themselves arise from

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Pl. II. Figs. 3, 4, 5. † Pl. II. Fig. 7. ‡ Pl. II. Fig. 17. § Pl. IV. Fig. 11.

the inferior edge of this part, and are about sixteen or seventeen in number; they are not laminated like those of the higher Crustacea, but consist of one large piece, which is apparently composed of a great number of cells.

The organs of generation are not apparent in the male, but in the female, and, especially when she is loaded with spawn, these organs are at once perceptible. They are very similar in their structure and appearance to the same parts in the female *Mysis*. They consist of four scales, which arise from the inferior edge of the thoracic segments. These scales are of an irregular oval shape, concave internally, and convex externally, and they are overlapped by one another.* The eggs are of considerable size, and of a bright straw colour. It is from the genus *Cuma* only that these observations were taken in regard to the organs of generation.

When a portion of the skin, or shell rather, is placed under the microscope, it presents a very beautiful appearance; it apparently consists of a great number of nuclei, arranged in some degree of order. These nuclei are stellated, and here and there larger nuclei may be observed, the edges of which are quite smooth.†

The structure of these animals is so peculiar, as to render the assignation (at present) of a proper place in a natural arrangement of the class, a point of very considerable difficulty. This arises in a great measure, without doubt, from our very limited knowledge of the class. I rather think, however, that they should be ranged among the lower *Decapoda macroura*.

Genus CUMA (*Edwards*).

Generic Characters.—The superior antennæ are single-jointed, and scale-like; the inferior antennæ are five-jointed. The caudal styles have the double terminal scales biarticulate, the last of which is always the shortest.

Cuma Edwardsii, mihi.‡

C.—With the superior antennæ rhomboidal; with the ambulatory division of the first pair of legs, with the first joint bent at an obtuse angle; with the thumb-like process single-jointed, and with the last joints clavate. Length, 4 lines. Hab. Frith of Forth.

Description.—The whole animal is of a fine straw colour, with a delicate tinge of pink, which is brighter in certain lights; the shell is quite

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* Plate IV. Fig. 12. † Plate II. Fig 18. ‡ Plate II. Fig. 1.

rough, which is caused by the great number of shallow foveæ with which the whole surface is thickly covered. This, and the following species, are perhaps the smallest of the genus ; at the same time, they are much thicker and stronger in proportion to their size than the other species. The rostrum is short, thick, and suddenly truncated obliquely. The antennæ are minute ; the first or superior pair are almost obsolete ; they consist of one joint only, which is rhomboidal ; the extremity of each is armed with several strong but minute hairs or spines ; they arise from the truncated extremity of the rostrum. The inferior antennæ* arise from the inferior surface and base of the rostrum ; they are considerably larger than the superior pair ; they are five-jointed, the third joint being the longest, the fifth or last is extremely small, and is armed with three very strong pointed and articulated spines. These pair of antennæ are somewhat longer than the rostrum. The foot jaws are rather powerful, and have a great resemblance to the following pairs of feet. The first, or superior pair, are the smallest ; the first joint is of considerable length, being equal to all the others combined ; it is rather bent and broad, and is armed at its distal extremity with two thumb-like processes or tubercles. Two very long and slender spines, which are almost as long as the foot-jaw itself, arise from the middle part of this segment ; the external spine is free of spinules altogether, but the internal is armed, on its external edge only, with a great number of articulated spinules. The second segment of this foot-jaw is very short, and its posterior edge bears two very short articulated spines of equal length ; these spines are spiniferous. The third segment is almost equal in length to the first, and, like the second, also gives rise to nine or ten articulated and spiniferous spines. The fourth segment is small and rounded, being also armed on its posterior edge with simple spines. The fifth segment is thumb-like, and spinous on its posterior edge.

The external pair of footjaws are much larger than the internal ; they are five jointed, and are armed in the same way as the first pair, except that the external edge of the first segment is armed at regular intervals with small tufts of very fine hairs ; the extremity of the second segment is also armed with a very long articulated and spiniferous spine. These two extremities just described are in general lying in such a way as to cover the organs of the mouth.†

The two first pairs of legs are constantly concealed beneath the carapace when the animal is at rest, covering the footjaws and the organs of the mouth, and appear only to be used when the animal is swimming. The anterior or ambulatory division is five-jointed ; the first joint is about twice the length of all the others combined ; it is considerably bent and very broad ; its internal edge is armed at regular intervals with pennicillated tufts of hair ; the three following segments are quite free of spines, but the last is armed at its extremity with a strong claw and two smaller spines. An articulated thumb-like and chelate joint

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* *Plate II. Fig.* 5.

† Plate II. Fig. 7.

rises from the extremity of the first segment, immediately internal to the four last segments. The natatory or posterior division of this leg is multiarticulate; the two first segments are longest, being equal in length to the first segment of the anterior division; the remaining segments are minute, about nine or ten in number, each of which gives off a very long spiniferous setum, which is articulated at its distal half.* The second thoracic leg of this species presents to us one of those beautiful and delicate structures which it is impossible either to describe or delineate with even a remote degree of accuracy. The ambulatory division is very long and slender, six-jointed; the first joint is long and very much flattened, but tapers from the middle towards its distal extremity, which is armed with a very long and pointed spine; the following joints are all equal to one another in length, except the last, which is minute. The natatory division of this leg is seven or eight-jointed, and is equal in length to the first segment of the other division. The five last segments are all armed with long articulated and spiniferous setæ, which smaller spines are again spinulose.† The four following pairs of legs are simple, that is, they are merely ambulatory; they are all six-jointed, and are very spiny. The segments of the body from which they arise are all ovoid, their dorsal edge being sharp and pointed.‡

The abdominal portion of the body is long and slender, seven-jointed and moniliform; the last joint is minute, and lies between the caudal styles which arise from the extremity of the sixth segment; these styles are of no great length in this species; they are composed of three parts; each style consists of a long jointed peduncle, from the distal extremity of which two biarticulated scales arise; these scales lie one above the other. The first segment of the peduncle is somewhat longer than the sixth abdominal segment; the first segments of the scales are about half the length, and the last segment about one-fourth the length of the peduncle; the inner edge of the superior scales is armed with a number of long, pointed, and articulated spines. The spines which arise from the inner edge of the inferior scales are more numerous; they are all bent, their points being turned backwards; the convex or anterior edges of all these spines are very much serrated.‖

I have named this species after M. Edwards, the founder of the genus, and the leading crustaceologist of the day.

Cuma Audouinii. Edwards.§

C. With the superior antennæ very small; with the first joint of the ambulatory division of the first pair of legs almost bent at right angles; the terminal joints oval, and the thumb-like process multiarticulate. Long four lines to five. Hab., Frith of Forth.

Description.—Under casual observation this species is very apt to be mistaken for that last described, but by careful examination the difference is

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* *Plate II. Fig. 10.*

† Plate II. Fig. 9.

‡ Plate II. Figs. 11, 12.

‖ *Plate II. Fig. 13.*

§ Plate II. Fig. 13.

found to be very material. In its general appearance, this species resembles the *Cuma Edwardsii.* The first thoracic segment, however, is longer and not so rounded; the rostrum is shorter and more pointed, and the eyes are larger; the flattened surface on the sides of this species is not so decided. The second thoracic segment is more hid; the third is larger, ovoid, and rounded; the adjoined scale projects backwards; the fourth segment is of the same shape as the third, but not nearly so large; the fifth ends in a sharp point, both superiorly and inferiorly; the sixth thoracic segment is clavate. The superior antennæ are very small, and scarcely to be distinguished from the rostrum. The inferior antennæ are very similar to those of the *Cuma Edwardsii.* The foot-jaws are also similar in their structure to those of the last described species; the ambulatory division of the first leg is five-jointed; the first-joint is very much bent, and is of considerable breadth; the two last joints are quite oval, and the last nonchelate. The internal thumb-like process, instead of being composed of one-joint only, as in the last described species, consists of four or five segments, which are all armed with short spiniferous and pointed spines; the natatory portion of this leg is multiarticulate, the extreme joints being very small, so as to place the long spiniferous setæ very close to one another.*

The second pair of legs are very short.† The four last pairs of legs are similar in their structure to those of the last described species. The abdomen and caudal fins also bearing a similar resemblance.

This species is apparently the Cuma Audouinii of M. Edwards, but whether it is or not I cannot be quite certain.

Cuma trispinosa, mihi.‡

C.—With the dorsal ridge of the caraface surmounted by three spines, with the ambulatory division of the first pair of legs extremely short, and with the second thoracic segment well developed. Long, 8 lines. Hab., Frith of Forth.

Description.—This is a most characteristic species, and brings out several points of material consequence in the character of the genus. This species has the body quite smooth, and of the same colour as the preceding. It is the largest of all the species, but is more slender. The thoracic segments are not so deep as those of the preceding species, and the lateral compression is awanting. The rostrum is sharp-pointed, and bent considerably upwards; the eyes are small, and the dorsal ridge immediately behind the eye is surmounted with three thick short spines. The second thoracic segment is of considerable extent at its dorsal part, but is quite obsolete at the middle; it again, however, makes its appearance at its inferior part, where it supports the second pair of compound legs. The four following segments gradually decrease in size:—The superior antennæ are of considerable size, oblong and spinous. The inferior antennæ are much longer than the rostrum. The ambulatory division of the first pair of legs is extremely short, and the first joint is of no great breadth. The natatory division is about the same length as the first joint of the anterior division.[*]

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* *Plate II. Fig 19.*

† Plate III. Fig. 1.

‡ Plate IV. Fig. 16.

The second pair of legs are very long and slender; the first segment is not broader than the following joints, and is armed internally at its extremity with a very long spine.[†]

The simple feet are extremely spiny.[‡]

The abdominal portion of the body is very long and slender, the fifth segment being the longest. The caudal styles are long, slender, and pointed; the internal scale has the last joint pointed and armed with two spines; the last segment of the external scale is more obtuse.[||]

Genus ALAUNA, mihi.[1]

Generic Characters.—The superior antennæ are composed of a peduncle and a multi-articulate filament. The inferior antennæ are eight-jointed. The three first pair of legs are compound. The internal scale of the caudal style is composed of three segments, and the external of one.

Alauna rostrata, mihi.

Description.—The whole animal is of a beautiful bright straw colour, inclining to yellow. The thoracic portion of the body is very large and swollen. The first segment or carapace is almost oval. The rostrum is long, pointed, and is bent upwards at its extremity. The eyes, which are of considerable size, are situated at the base of the rostrum. The superior pair of antennæ are very slender, consisting of a delicate filament covered with hairs, which arises from a short peduncle; these antennæ are almost equal in length to the rostrum.[2]

The inferior antennæ are much longer, consisting of eight joints slightly spinous; the distal extremity of the third is armed with a strong multi-articulate spine.[3] The foot-jaws are seen projecting considerably beyond the edge of the carapace; they are very spiny, and the last joint but one is armed with a long articulated spiniferous spine.[4]

The first pair of legs are extremely short; the thumb-like process at the extremity of the ambulatory division is single-jointed and spiniferous.[5] The second pair of legs are also short.[6] The ambulatory division of the third pair of legs is very long and slender, being almost as long as that of the second pair of legs; the fifth joint is the longest. The natatory division is as long as the first four joints of the ambulatory.[7] The simple legs are very spiny on their anterior edges.[8]

The abdomen is short and thick, seven-jointed, the last joint being produced into a long spine which is spiniferous on either edge; the anal aperture is seen near the base of this segment. The caudal styles arise from the sixth segment, and they are much more complicated than those of the foregoing genera. The first segment is slightly clavate, longer than the seventh abdominal segment, and armed with a single row of spines on its inner edge. The internal scale consists of one joint only; it is very spiny, and is about half the length of the external. The external scale is composed of three joints, the two first of which are equal in length to one another; the third is about twice the length of both of these, and is very spiny at its extremity.

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[*] Plate III. Fig. 3.

[†] Plate III. Fig. 4.

[‡] Plate III. Fig. 6.

[||] Plate III. Fig. 5.

[1] Plate IV. Fig. 1.

[2] Plate IV. Fig. 3.

[3] *Plate IV. Fig. 4.*

[4] Plate IV. Fig. 5.

[5] Plate IV. Fig. 6.

[6] *Plate IV. Fig. 7.*

[7] Plate IV. Fig. 9.

[8] Plate IV. Fig. 8.

Long, half-an-inch. Hab., Frith of Forth.

Having only obtained one specimen of *Alauna rostrata*, and one also of *Bodotria arenosa*, I have not been able to examine the structure of these two genera satisfactorily.

Genus BODOTRIA, mihi.

Generic Characters.—The first, second, third, fourth, and fifth abdominal segments are each armed with a pair of bifurcated finlets. The two terminal scales of the caudal styles are single-jointed.

Bodotria arenosa, mihi.

Description.—The carapace is almost oval, rostrum awanting, that part of the carapace being merely rounded off. The superior antennæ are quite obsolete. The inferior pair are of considerable length, and are terminated by means of two long spines.

The ambulatory division of the first pair of legs has the first joint of a very great size, being very much flattened and slightly curved. The four remaining joints, together with the internal thumb, are very spiny. The natatory division of the leg is six-jointed, the four last joints giving rise to as many long spiniferous spines, which are articulated at their distal halves. The external edge of these spines are spiniferous at the articulated half only. The ambulatory division of the second pair of legs has the first segment very broad, and tapering gradually towards its distal extremity, from which arises a very long, articulated, and spiniferous spine.

The abdominal finlets are five in number. They are composed of two parts, viz., the first or pedicle, and the second or bifurcation; the pedicle is of considerable length, from the extremity of which there arises two scales, which are armed on their margins with long spiniferous spines, which are much longer than the finlet itself.

The first segment of the caudal styles tapers very slightly, and the two terminal scales are each of them single-jointed, and end by means of very fine points. The external is armed at its extremity with two spines. Long, 5 lines.

This genus forms doubtless a link between the *Stomapoda* of M. Edwards and the higher Crustacea.

In their habits all these animals seem to agree. I have not been able to observe any thing peculiar in them. They swim with very great rapidity, and on stopping they fall to the bottom on the sand or gravel, without attempting to lay hold of —thing, as I have already remarked, seldom using their feet as a means of prehension. They free themselves with great dexterity from any weight which may happen to fall on them. I have often placed the point of a needle on their thorax and pressed them down into the sand; the animal immediately frees itself with very little apparent trouble, by means of its tail. The extremity of the tail is placed against the needle with one of the styles on either side of it, and by pressing upwards in this way, it soon regains its liberty.

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* Pla

They frequent sandy banks, and chiefly those where there is a little sea-weed.

DESCRIPTION OF THE PLATES.

Plate II.

Fig. 1. *Cuma Edwardsii.*
... 2, 3, 4, 5. Organs of the mouth.
... 6. Natural size of *Cuma Edwardsii.*
... 7. A superior and an inferior footjaw.
... 8. One of the inferior antennæ.
... 9. One of the second pair of compound feet.
... 10. One of the first pair of compound feet.
... 11. One of the first pair of simple feet.
... 12. One of the second pair of simple feet.
... 13. Caudal styles.
... 14. Enlarged view of *Cuma Audouinii.*
... 15. Natural size.
... 16. One of the first pair of compound feet of *C. Audouinii.*
... 17. One of the abdominal appendages of *Bodotria arenosa.*
... 18. Portion of the shell of *Cuma Edwardsii,* very much magnified.

Plate III.

Fig. 1. Enlarged view of *Cuma trispinosa.*
... 2. Natural size.
... 3. One of the first pair of legs.
... 4. One of the second pair of legs.
... 5. One of the caudal styles.
... 6. One of the simple legs.
... 7. Enlarged view of the carapace of the *Cuma trispinosa.*
... 8. *Bodotria arenosa.*
... 9. Natural size.
... 10. One of the first pairs of legs.
... 11. One of the second pairs of legs.
... 12. Enlarged view of anterior part of carapace.
... 13. One of the caudal styles.

Plate IV.

Fig. 1. ***Alauna rostrata.***
... 2. Natural size.
... 3. One of the superior antennæ.
... 4. One of the inferior antennæ.
... 5. Enlarged view of the anterior part of the carapace, with one of the footjaws projecting from its anterior edge.
... 6. One of the first pair of compound legs.
... 7. One of the second pair of compound legs.
... 8. One of the simple legs.
... 9. One of the third pair of compound legs.
... 10. Caudal styles.
... 11. Branchiæ of *Cuma Edwardsii,* with one of the footjaws attached.
... 12. One of the second pair of compound legs of *Cuma Audouinii,* with the ovarian scale attached.

Description of a Self-Registering Tide-Gauge, invented by Mr JOHN MAXTON, Engineer, Leith.* With a Plate. Communicated by the Royal Scottish Society of Arts.

The machine represented in Plate V. was designed by me for registering the amount of tidal rise at any point on the coast, as at a sea-port or navigable river, or in any situation where it is of importance to ascertain the whole rise of the tides for a length of time. Several instruments have been invented for this purpose, and some of these are now in use, it is believed, both in this country and in France ; but that which I have invented, and am now to describe, seems more simple in its construction, and promises to be, at least, as well, if not better, calculated to effect the object for which it is intended, than any other construction that has come under my notice.

In figs. 1 and 2, *a* is a plate of $\frac{5}{8}$ of an inch thick, with dovetailed feathers *b b,* on its surface, between which are grooves represented by the dark spaces, $\frac{1}{2}$ inch in depth ; in these grooves are placed moveable *studs, c c,* which are made to slide easily along the whole length of the grooves, and before the machine begins to operate, the whole of them are set near the centre of the plate in two lines, as shewn in the upper part of fig. 1. Each groove represents the rise and fall of a tide ; and there being two tides in the twenty-four hours, two of these grooves are employed in registering one day's tides. From the mechanism of the machine the studs on the right hand of the centre or zero line are for registering the height; and those on the left the lowness of the tides as measured from the half-tide level. The figures on the right and left margins correspond to the days of the month; and the drawing represents a register for twenty-eight days' tides, or one lunar month. In figs. 1 and 3, *f* is a pulley with a cord or small chain passing round it; to one end of the cord is attached a float *g* (fig. 3.), and to the other end of the cord is a weight *h* (figs. 1 and 3), which acts as a counter balance to the float. On the axle of the large pulley *f*, is a pinion *x*, and the smaller the diameter of the pinion is, in proportion to that of the pulley, the narrower and more compact the registering plate or table (fig. 1) will be. Letter *j* represents a rack; the number of teeth and revolutions of pinion *x*, during the whole range of tide, determining the length of the rack and the proportion of the scales of feet and inches at the top and bottom of the registering plate (fig. 1.) Connected with the horizontal rack *j*, is a vertical guide or traversing bar *l*, which is made to move the whole breadth of the table by its rack and the pinion. At the top and bottom of the vertical bar are pullies *m*, for running along the guide-rods *n*. In the vertical bar there is a groove, in which the sliding bush *z*, is made to move freely up and down; to this bush is attached a cord, passing over the pulley *p*, at the upper end of the bar, and a constant strain is kept on the cord over the pulley by a weight *q*, to prevent the bush *z*, from falling downwards. In the bush *z* is a pin which projects into the dovetailed grooves, between the feathers *b*, and slides easily along in them, as the bar *l* traverses either way. This pin moves the studs *c c*, to their proper places for indicating high and low tide. Letter *r*, as will be explained presently, represents moveable tongues or switches, having joints at one end, so loose, that when lifted they will fall down again by their own weight.

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* Read before the Royal Scottish Society of Arts, 28th Nov. 1842.

We shall suppose that the machine has registered the tides as far as the second tide, on the 9th of the month, as shewn in the diagram (the studs below this being all shewn as moved to their places, and those in the upper grooves remaining unmoved), and that the tide on the 9th has fallen 7 feet from the datum line (marked *o* on the scale), to this position, therefore, the pin in the bush *z* has moved the sliding stud from the original position in which it was set. Supposing the tide began to flow when the machine was in this last position, by the float *g* (fig. 3), rising, it would reverse the motion of pulley and pinion, and bring the rack and traversing bar towards the right, or towards high water, on the table. After having left the sliding piece at its position for denoting low water on the 9th of the month, it is now proceeding towards the sliding piece for denoting high water on the 10th; and when the bush and pin come to the tongue or switch, the pin moves up the inclined plane and on towards the right, moving the sliding piece for denoting high water on the 10th to its right position for that tide. Supposing now the tide to ebb, the action of the float reverses the wheel, pinion, rack, and traversing-bar, and when the bush and pin come to the under side of the tongue, towards the left, the pin will lift the tongue by the strain produced by the weight *q*, on the cord which is attached to the bush; and having lifted the tongue, and passed on in a straight line, the tongue falls immediately by its own weight after the pin in the bush *z*, has passed it; and coming back for the next high water, the pin has to move up the inclined plane as before, and so on with the whole of them.

The snugs *s*, are for fixing the machine securely by screws to any convenient place for its reception.

There is another way that might be adopted for the float giving motion to the machine than a cord and pulley (see fig. 4.) A vertical rack *o*, attached to the float to work a spur-wheel *y*, which could be of the same diameter at the pitch-line as the diameter of the pulley *f*, so as not to derange the other parts and scales. The vertical rack might be more correct in the event of a cord being apt to stretch, which, however, would be obviated with a chain; but for high tides, say 20 or 21 feet, a rack would be very unwieldy, for it would require to be equal in length to the highest tides.

The full size of the registering part of the machine is about 2 feet square over all, and 2½ inches in depth; and if made of brass (as iron is apt to corrode from the action of the moisture from salt water), the cost of the whole apparatus, including the float and counterbalance, and the pipes in which they work, I have estimated at about L 30.

JOHN MAXTON.

LEITH ENGINE-WORKS, 17*th Nov.* 1842.

Historical Remarks on the first Discovery of the real Structure of Glacier Ice. By Professor Forbes, Corresponding Member of the Royal Institute of France.

I feel myself most reluctantly called upon to state some circumstances respecting the discovery of a fact in the theory of Glaciers which M. Agassiz has declared, in a paper printed in the last number of the Edinburgh Philosophical Journal, to be erroneously claimed by me.

The first account of "*a remarkable structure of the ice of glaciers*," by myself, was printed in this Journal for January 1842. A history of this discovery, entirely opposed to mine, appears at pages 265 and 266 of the last number. By the kind permission of the Editor, I have now the opportunity allowed me of stating how the facts really stand, and at the same time of explaining the circumstances under which the publication of the original paper, claiming the discovery, took place,—circumstances which delicacy prevented me from mentioning at the time, but which it now appears essential to make known.

Private report, proverbially exaggerates and misrepresents the history of transactions little interesting to any but those immediately concerned. I believe that my own conduct and its motives have been misunderstood, with reference to the matter in question. A few extracts from the ample correspondence of which I am possessed in illustration of every step of the transaction, will, I hope, suffice to place the matter clearly before such readers as shall feel sufficient interest to follow them. I pledge myself to their accuracy, and to their being fairly extracted in conformity with the tenor of the letters to which they belong. If any doubt shall be raised on this point, I shall have only the disagreeable alternative of publishing the entire correspondence, the length of which would render it unsuitable for the pages of a scientific journal. But I repeat my belief that the extracts I shall make, and the narrative with which I shall connect them, will put the matter in a light sufficiently clear; and for the facts which I shall have to state, I am conscious of their admitting of no colouring or denial.

In the *first* place, I shall briefly state the circumstances under which the observation of THE VEINED STRUCTURE IN THE ICE OF GLACIERS* was made.

In the *second* place, I shall explain the circumstances under which I made it public

In the *third* place, I shall discuss shortly the claims to priority of observation which have subsequently been made.

I.

In 1840, M. Agassiz invited me to make a tour with him the next summer amongst the glaciers of the Oberland, Vallais, and Savoy. I understood the invitation to extend simply to our mutual companionship on a journey of mutual interest. Of third parties there was no mention; and it was with diffidence that I requested permission for my friend and fellow traveller, Mr Heath, fellow and tutor of Trinity College, Cambridge, to increase the number. It was only after all preliminaries were arranged, and after I had agreed, in order to accommodate M. Agassiz, to change the direction in which I proposed to commence our intended tour, that I learned that he had several friends in company with him; and it was not until my arrival at the Grimsel, on the 8th of August, that I learned that the plan of a tour, into which I had originally gone, had been abandoned by my fellow-traveller, for reasons which he did not assign, and that I was expected to unite with the party he had formed at Neufchâtel, to spend some time on the glacier of the Aar, instead of prosecuting the journey originally proposed. I cheerfully acquiesced, however, in the arrangement, which promised to give me a good insight into the structure of glaciers, which I proposed farther to study by prosecuting alone, or with Mr Heath, my originally projected tour to Monte Rosa and Mont Blanc.

It is to be remembered that the glacier of the Aar was the one which M. Agassiz had already repeatedly visited in former years, and on which he had constructed a sort of hut in which he had lived for some time.

His other friends not having all arrived, M. Agassiz, Mr Heath, and myself, accompanied by (I believe) a single guide, ascended the glacier on the 9th August 1841.

See Edinburgh Philosophical Journal, January 1842, p. 89.

Fact 1. We had not walked for half an hour on the ice, when I directed the attention of my companions to what I called a *vertical stratification* pervading the ice. It appeared to me so plain, that it scarcely occurred to me that it could be new to M. Agassiz, who had so often traversed the same ground.

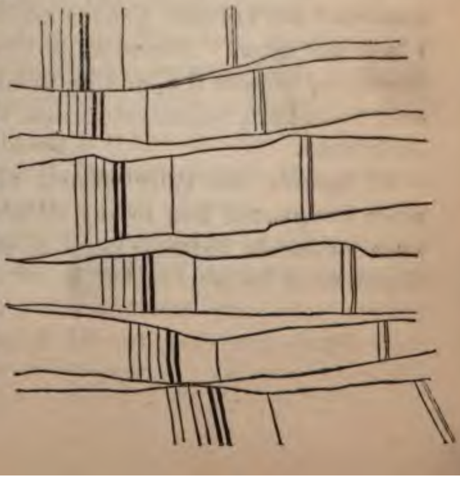
Fact 2. M. Agassiz having his attention called to the fact, stated that he thought I was deceived in considering that it penetrated the ice ; that, indeed, the surface of the glacier seemed to him much changed since last year, but that he had observed *superficial linear markings* of the same kind on (I think) the Glacier du Bois.

Fact 3. At each new *crevasse* we came to, I took pains to shew him that the apparent strata penetrated into the mass of the glacier ; but he seemed incredulous, until I noticed a deep hollow in the ice close to the left margin of the medial moraine between Hugi's and Agassiz' cabins, at least 20 feet deep, to which I called M. Agassiz' attention, in proof of the position I had maintained.

Fact 4. To this he assented, but expressed his belief that it would only be found in the neighbourhood of the moraine, and not throughout the breadth of the glacier.

Fact 5. In the course of the same afternoon, we ascertained, by conjoint inspection, that the structure in question was traceable *all across* the glacier of the Finster Aar.

Fact 6. M. Agassiz, unwilling to admit that he could formerly have overlooked so palpable a structure, expressed a frequent doubt whether this structure had not been superinduced since his last visit.



Fact 7. I took the following means of proving that this could not be the case. I shewed him some *crevasses*, and asked him how old he supposed them to be ? He answered, several years ; they certainly had not opened since last summer (1840.) I *shewed that the veined structure crossed these* *crevasses*, and was dislocated by them, as in the margin, and, therefore, must have been anterior to their formation.

Let us hear the evidence of Mr Heath and M. Agassiz, the only witnesses present besides the guide.

Mr Heath wrote to me thus, on sending him the above statement of facts:—

EXTRACT FIRST.—*Rev. J. M. Heath to Professor Forbes, (printed by Mr Heath's permission.)*

TRINITY COLLEGE, 8*th March* 1842.

" * * But those who were there this summer have very different evidence that this was a new fact. I remember when it was first remarked, Agassiz said he had seen it before, but not to such an extent. That it had a peculiar relation to the medial moraines, and would not be found in the centre of the glacier; that it was *only superficial,* and owing, as he believed, to the sand which placed itself in parallel straight lines, and produced these incisions by melting the ice. The afternoon was taken up in what I then thought a very superfluous endeavour to make out whether it was superficial or not, and I believe he maintained the contrary opinion until the discovery of the great hole of which you have given a drawing."

It will be observed, then, that the whole question lies in this, Whether the lined appearance of the ice was due to an inequality of melting, occasioned by a linear arrangement of sand on the surface, washed from the moraines, and intercepting here and there the sun's rays?—or, Whether it was occasioned by the unequal action of the weather on alternating vertical bands of friable and of compact ice, of which the glacier is composed. M. Agassiz appears, upon Mr Heath's testimony and my own, to have taken the former view, whilst I took the latter. According to him, the ice was striated *on its surface,* because the sand lay in lines; according to me, the sand lay in lines, because the ice has a veined structure *throughout its mass.*

M. Agassiz, the other witness, admitted as much himself, when I requested him to say whether the above-cited facts were accurately stated or not. In a letter to me, dated 29th March 1842, he says,—

EXTRACT SECOND.—*Professor Agassiz to Professor Forbes, 29th March* 1842.

"Comme vous en convenez vous-même lorsque nous discutâmes pour la première fois les bandes de glace de teintes diverses que l'on observe dans le glacier, *je vous dis que j'en avais remarqué* DES TRACES SUPERFICIELLES au glacier des Bois en 1838, ce qui est mentionné dans mon livre p. 121, à l'occasion des moraines médianes."

It appears, then, that Mr Heath's memory and my own agree thus far precisely with M. Agassiz'. Let us see whether the reference to the "Etudes sur les Glaciers," published in 1840, gives any farther evidence.

EXTRACT THIRD.—*Agassiz, Etudes sur les Glaciers, p.* 121-2.

"Les trainées régulières et parallèles de grains de sable que l'on poursuit quelquefois sur de très grandes étendues, le long des moraines médianes, me paraissent être un effet de la dilatation de la surface chargée de debris, combiné avec le mouvement progressif de toute la masse. Les petits grains de sable épars, n'agissant pas comme les gros blocs,* tendent à former des series [Qu. *stries*?] longitudinales et parallèles qui se transforment quelquefois en rainures, et qui servent même souvent de lit aux petits filets d'eau qui coulent le long des moraines. Nulle part je n'ai observé ce phénomène d'une manière aussi frappante que sur la Mer de glace de Chamonix en 1838 ; je l'ai également remarqué sur le Glacier de l'Aar, et ce qui m'a confirmé dans l'explication que j'en donne, c'est qu'ici on remarque sur le côté gauche de la grande moraine une petite moraine qui lui est parallèle, et qui me parait détachée de la même manière que les trainées de sable dont je viens de parler se détachent des moraines en général."

It appears then, that, after three years of observation of the glaciers, M. Agassiz still entertained, in 1841, the same view of the cause of a fact which he had observed in 1838, and published in 1840. The *fact* was the superficial arrangement of lines of sand near the moraines of glaciers, which, according to him, arose from some molecular dilatation of the ice, which he does not very clearly explain ; and its *effect* was *sometimes* to produce grooves (*rainures*), by the heat of the sun acting on the sand thus arranged.

The fact which I pointed out to him on the 9th of August had no reference to the arrangement of sand on the ice, but consisted in a *texture* which the ice itself presented throughout its mass, of harder and softer layers, whose wasting, when it occurred in the neighbourhood of the moraines where the glacier was covered with sand, occasioned hollow grooves, into which, for obvious reasons, the sand was speedily washed, and there it lay. M. Agassiz was very naturally and properly slow to admit, in explanation of a fact which had for three years been before his eyes, the existence of a prevalent structure to which he had not adverted. Accordingly, his convictions were proportionably gradual; and, as Mr Heath observes, " the afternoon was taken up in what I then thought a very superfluous endeavour to make out whether it was superficial or not."

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* This refers to the well-known action of large blocks of stone in defending the surface of the ice from evaporation ; here, on the other hand, *the sand sunk in the ice.*

Two days after the discovery of the structure, namely, on the 11th of August, we were joined by Professor Studer, the distinguished geologist of Berne, and by other friends of M. Agassiz. The structure in question having been discussed, it is important to know the impression which it left as to novelty or originality upon the mind of so competent a judge. M. Studer writes to me:—

EXTRACT FOURTH.—*Professor Studer to Professor Forbes,* 19*th March* 1842. *Extracted by M. Studer's permission.*

" M. Desor* m'a écrit il y a quelques semaines de cette contestation de priorité; je lui ai repondu que je ne me mêlerais pas de cette affaire, mais que bien certainement vous m'aviez fait remarquer pour la première fois la structure en question, et que j'avais cru en effét que son importance avait échappée à Agassiz, comme à tous ses devanciers."

I will only cite one other testimony as to the origin of the discovery on the Glacier of the Aar, also by an eye-witness, Mr Robertson of Newton House, near Elgin, a friend of M. Agassiz, whom I did not know before, and whom I have not seen since, but who, having learnt the nature of the contest as to priority which has occurred, generously and voluntarily sent me the following statement of facts, which I have likewise his permission to publish.

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* A friend of M. Agassiz.

EXTRACT FIFTH.—*Mr Robertson of Newton to Professor Forbes.*

NEWTON, *4th May* 1842.

"Before joining you on the 13th August last year, I was pretty familiar, from reading, with all the ordinary phenomena of glaciers, and, on my walk to the 'Cabane,' examined each as it presented itself. Among others I observed the *superficial* indications of the ribboned structure; and, during the first half hour after my arrival, I recollect perfectly, in walking from the 'Crevasse' at the end of the Finster Aar glacier (where you had been preparing the experiment on the absorption of ice with red wine) to the left flank of the Lauter Aar (where we exposed, with a hatchet, the contact of the ice and rock, in order to see the sand, &c. between them), having asked Agassiz how it was produced? He told me that the surface of the glacier had completely changed since last year, when he had *scarcely* observed it,—that it was an effect of the moraines, and probably caused by the greater variations of temperature to which they were subject as compared to the rest of the glacier, and that it had nothing to do with stratification. I remember also asking whether the horizontal lines at the end of the glacier were those of stratification? and was told 'undoubtedly.'

"On our return to the 'Cabane,' I pointed out the structure very well marked, at some distance from the moraines, and, on cross questioning Agassiz, saw that he was far from satisfied with his theory.

"I have thus abundant evidence, independent of your ample testimony, to shew, that, at the date I have mentioned, my friend Agassiz was unaware of the general occurrence of the ribboned structure, through the mass of glaciers; and, in writing to him some days ago, mentioned my conviction that the discovery, certainly the most important of the recent ones, was due to you. I shall be glad to find that, as I believe is the case, M. Desor alone, and not M. Agassiz, could call it in question."

The "stratification" alluded to at the close of the first paragraph of the preceding letter, refers to the twisted planes of structure which I have described in my paper, and which are, in fact, continuous with the veins which, throughout the greater mass of the glacier, run parallel to its sides, when these sides are steep and continuous. The complex form of the surfaces of the shells into which a glacier is divided by these bands of compact and friable ice, I was first able to discover, during a visit to the glacier of the Rhone on the 23d August 1842. ① I was accompanied by Mr Heath, and Mr Calverley Trevelyan, but not by M. Agassiz or any of his party. In the course of a very careful examination of the glacier, I succeeded in satisfying myself completely of the conoidal form *of the veined surface*, and in explaining the apparent frontal stratification, which I have since confirmed in every point.* On our return to the Grimsel, I explained my views to M. Agassiz, who copied the sketch I had made, which corresponds exactly to that in the Edinburgh Philosophical Journal, January 1842, p. 89. A month later, I explained this system of curves of structure of the glacier of the Rhone to M. Studer at Berne. His penetration immediately perceived its importance, and he expressed great satisfaction at the insulated fact which I had pointed out to him on the glacier of the Aar being thus generalized.† We both agreed that its explanation must involve, in a good measure, the true theory of glaciers. In a letter to Professor Bronn of Heidelberg, dated 1st October 1841, a week after I had quitted Berne, M. Studer gives an accurate account of my observations, being the first publication on the subject.‡

II.

I now come to state shortly the circumstances which led to the publication of my paper describing this new structure of glacier ice; and about which there seems to have prevailed a misapprehension which I am anxious to remove.

It has been supposed that I resisted every offer to take a share in a joint publication of the proceedings of the summer, in order to bring forth a separate notice of the structure which I had observed; that even whilst in Switzerland, I contemplated such a separate publication; and having reached England, hastened to anticipate M. Agassiz.

The facts are precisely the reverse. The idea of publishing either this or any original observation of my own, on a subject so new and so unexpectedly difficult as I found the glacier theory to be, had certainly not entered my imagination during any part of my stay abroad. A *precis* of the labours of *others* in the form of a Review of the writings of Venetz, de Charpentier, and Agassiz, such as subsequently appeared in the Edinburgh Review, I certainly contemplated, thinking, that if I pursued the subject another year, such a preliminary study would be the fittest introduction to any original investigations. But I can safely say, that the way and manner in which my observations on the glacier structure should be brought out, was not a matter of the slightest concern to me, until an unexpected circumstance brought it to my mind.

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* See Letters to Professor Jameson in this Journal for October 1842, p. 346.

† M. Studer, after quitting the glacier of the Aar, had recognized the structure on several others in the canton of Vallais. I should add that I pointed out the veined structure to M. Agassiz on the glacier of Gauli, in the Urbachthal, on the 20th August, and it was afterwards noticed by both of us on the Oberaar glacier, and that of Aletsch. So that no reasonable doubt remained, at least, on my mind, that, having been observed on no less than five contiguous glaciers, it was a general and not a particular phenomenon. This meets M. Agassiz' statement, that I not only "erroneously claimed the discovery," but "assigned to it a generality which the facts observed by myself did not at all justify."—*Ed. Phil. Jour.*, p. 265.

‡ *Leonhard's Jahrbuch*, 1841.

I must mention, however, what passed between M. Agassiz and myself relatively to a joint publication, when I was at Neufchâtel in the middle of September 1841. I will state this in the words which I employed in writing to a friend a few months after the transaction took place.

EXTRACT SIXTH.—*From a Letter from Professor Forbes to a Friend, dated 1st April 1842.*

"M. Agassiz never asked me, so far as I recollect, to publish with him on the subject of the Glaciers. He once proposed to me to communicate the observations I had made on Solar Radiation on the Glacier of the Aar, to form part of the description of the journey, of which the narrative part was to be written by Desor.

"This I declined, on the ground that these observations formed part of a series of experiments, long since commenced, and which must be treated of in connection.*

"I was very well aware, however, that a declaration of my opinion on the Glacier Theory was what was desired; and M. Desor took upon him to intimate this to me at Neufchâtel, in these words:—'M. Forbes ne veut pas se compromettre, mais nous le compromettrons'—which you will think rather a singular way of securing support to a scientific dogma. The following reasons determined me against taking any part in a joint publication:—

"*1st,* That however willing I might be to have my name associated with that of Agassiz, in any common work, experience led me entirely to decline such an association with M. Desor.

"*2d,* That the utmost extent to which I could then conscientiously have gone in support of the Glacier Theory, would, I knew, not have satisfied M. Agassiz.

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* They *accordingly form part of* a very extensive enquiry since communicated *to the Royal Society of* London.

"*3d*, That, from the perusal of Charpentier's work, and from communications with those best acquainted with the history of the Theory in Switzerland, I had begun to perceive, that were I to take any part in the discussion going on between Agassiz and Charpentier, it must be in favour of the latter, and not of the former, as an original observer and just reasoner."

I reached home in the month of October 1841, and soon commenced the Historical Review of the Glacier Question which I had projected. Whilst I was thus engaged, the *Comptes Rendus* of the Academy of Sciences at Paris for the 18th October reached me. In it I found a letter from M. Agassiz to Baron Humboldt, containing the following passage, with reference to the observations made on the Glacier of the Aar.

EXTRACT SEVENTH.—*From Professor Agassiz to Baron Humboldt.*

"Le fait le plus nouveau que j'ai remarqué, c'est la présence dans la masse de la glace de rubans verticaux de glace bleue, alternant avec des bandes de glace blanche d'un quart de ligne à plusieurs pouces de large, s'étendant sur toute la longueur du glacier, c'est à dire, à plusieurs lieues de longueur, et pénétrant à une profondeur d'au moins 120 pieds puisque j'ai observé encore ce phénomène au fond du trou de sonde."

On reading this letter, from which all mention even of my presence on the glacier of the Aar is excluded, my first impression was of surprise and pain. That I could not suffer so direct a plagiarism to remain unchallenged never appeared to me to admit of doubt; *le fait le plus nouveau que* J'AI *remarqué*, was an assertion as articulate as it was unfounded. How to take notice of it was a point of more difficulty. I felt fully the delicacy of my position. Towards M. Agassiz I felt the warmest friendship; sympathy with his zeal, and gratitude for his kindness and hospitality. This he well knew: during several weeks of the closest intimacy, we had been perpetually engaged in discussions connected with his theoretical views, and also respecting facts. I believe it may safely be stated, that neither of us ever for a moment lost temper in these amicable disputes, which often lasted for hours together, and which were uninterrupted either by our walks or our meals. His enthusiasm and good temper in these discussions *delighted* me, even where he failed to convince me of the constancy of his alleged facts, or the cogency of his reasons. We parted at Neufchâtel with even more cordiality (at least on my part), than we had met at the Grimsel; and my letters written afterwards testify that I freely acknowledged my obligations. Accordingly, in vindicating the originality of my observation, I resolved to take the plan which seemed to me most likely to secure a continuance of a friendship I so much valued. Were I to write to complain directly of want of justice on his part, though I did not doubt his willingness to correct his error, I felt that it would place him in a somewhat painful position, after so direct an assertion of his own rights. I preferred a different, and, I think, a natural course. Knowing well the facts stated in the commencement of this paper, and feeling that M. Agassiz must be equally aware of their truth, I resolved to make no reclamation, and especially to testify in my letters no irritation at the part which he had taken; but simply in a short and matter-of-fact communication to the Royal Society of Edinburgh, which I lost no time in transmitting to him, to state my own version of the affair, and claim my discovery without the slightest allusion to its having been erroneously claimed elsewhere. This was the origin of the paper which, at Professor Jameson's request, was communicated afterwards to his Journal; and any one who looks at it in this view, will, I think, admit that it was well calculated to answer the end proposed. It has not a trace of a controversial character, but I well knew that when it should meet M. Agassiz' eye, it would be understood as an intimation that when he should next publish, I expected my claims to original observation to be more carefully regarded, though, in consideration of our friendship, and of the informal character of the communication to M. de Humboldt, I was both willing and happy to dispense with any apology.

At the same time that I communicated my paper to M. Agassiz, I sent it to Mr Heath, the only other party to the observation of the 9th August,—referring to the letter to Humboldt as the cause of the publication, and requesting his friendly opinion as to whether I had acted prudently in thus asserting my claim, and whether he considered all that I had stated to be justly my due. To this letter I received the following reply, which is here printed with Mr Heath's kind permission.

EXTRACT EIGHTH.—*From the Rev. J. M. Heath to Professor Forbes.*

TRINITY COLLEGE, 25*th Feb.* 1842.

"I am very much obliged to you for the extract from the Philosophical Journal. I saw the paper in the Journal before you sent it me, and I most cordially approved of its appearing. I did not know, what you seem to say in your letter, that Agassiz has claimed the main observation as his own. * * * I will witness that, 1*st*, He knew nothing about it; 2*d*, When he did see it, he said it was superficial, and caused by superficial *sand*; 3*d*, That he was the last to believe that it went to any depth. I think your account very true, and not claiming one jot more than fully belongs to you."

I certainly anticipated that my forbearance with respect to M. Agassiz would have been rightly interpreted, and that silent acquiescence would have acknowledged the justice of my claim.

The event proved otherwise. The particular steps which were taken by M. Agassiz to vindicate what he professed to consider his due, arbitrarily and unexpectedly claimed in this paper of mine, were singularly in contrast to my conduct in the matter of Humboldt's letter, and to the usage in such cases. But *that* I am willing to pass over for the present, and I will now refer to the new claims of priority which he ultimately substituted for his own.

III.

We now pass on to the other claims to the priority of the observation.

About the same time that M. Agassiz claimed the observation of the Lamellar Structure of Glaciers, in the letter to Humboldt, he communicated verbally to the societies of Geneva and Neufchâtel the same fact; and though my information is not specific on this point, I presume that my name was not mentioned in connection with it. This I learn from my friend Professor Guyot of Neufchâtel, who, immediately on hearing the account of the observations on the Glacier of the Aar, recollected having observed and described something similar, three years before, on the Glacier of the Gries. The note containing this observation, and others connected with glaciers, had been read in 1838 in the presence of M. *Agassiz*, to the meeting of the Geological Society of France at Porrentruy, but was published neither at large nor in abstract. It appears to have dropped not only out of the records of the meeting, but from the minds of those who were present, since M. Agassiz, whom it was specially calculated to interest, takes no notice of it in his book, published two years later, containing his own observations, already quoted, on the superficial striæ; which he could not in common fairness have published without mentioning M. Guyot's contemporaneous and far more important observation of the *structure*, of which these striæ are only the outward indication, had he been acquainted with its true bearing, or, in truth, had he recollected it at all. Be this as it may, it seems that M. Guyot himself never repeated the observation, and, so far as it appears, never even *spoke* of it, between the meeting at Porrentruy in 1838, and his hearing, first at Geneva in October 1841, then at Neufchâtel in November, M. Agassiz' account of his "new fact." M. Guyot has most honourably testified to me[*] that *not one word had ever passed between himself and me*, which could have informed me of what he already knew on the subject; and, also, that he twice traversed the Glacier of the Aar, on the 18th and 19th of August 1841, without noticing or recognising the structure which he had himself described. I mention this, because M. Agassiz has thought it necessary to assume that the Glacier of the Aar was more distinctly veined in 1841 than in any of the previous years that he visited it, in order to account for his not having noticed it until he returned to the glacier in my company. In the Edinburgh Philosophical Journal for October last, page 266, he says,—"During the months of August and September 1841, this phenomenon was so well developed in the Glacier of the Aar, that *it could not fail to strike every observer*."

M. Guyot's next step was a perfectly natural and just one. Finding that his original observation had been totally forgotten, he reproduced his paper from his bureau, where it still remained in MS., and read it afresh before the Société des Sciences Naturelles at Neufchâtel, on the 1st December 1841, just five days before I was similarly engaged, not merely in claiming for myself, before the Royal Society of Edinburgh, the priority of observation to M. Agassiz, but likewise proving that he had his attention directed by me to the structure in question. The transaction with M. Guyot did not come to my knowledge until long after.

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[*] In a Letter dated 3d June 1842, in answer to mine in *Extract Tenth*.

Meanwhile, M. Agassiz sent no direct answer or complaint upon the receipt of my Paper on the Structure of Glaciers. I will not now advert to the means taken, through third parties, to discredit my statements, on the one hand, and on the other, to obtain from me a renunciation of my claim under a threat of exposure. Having no exposure to fear, I contented myself with sending to M. Agassiz a statement of the various *facts*, cited in the commencement of this paper, connected with the discovery on the 9th of August, requesting to know whether any of them, or which, were denied. A tardy and involved reply (29th March 1842) contained a denial of none of them, but (as we have seen, *Extract Second*) an exact confirmation of what both Mr Heath and I recollected him then to have stated respecting his own observations. But the real cause of the marked embarrassment of his reply I was not at the time aware of. He had now no apology for ignorance of M. Guyot's claim to prior observation, yet feeling that his own dissatisfaction with my publication was solely grounded upon my having claimed for myself something which rightfully belonged to *him* (M. Agassiz),—"*le fait le plus nouveau*" of 1841,—"les observations *les plus précieuses de la campagne*;" he naturally felt an embarrassment at being obliged to admit that similar facts and observations had been described in his hearing at Porrentruy three years before. Unable to maintain any longer his own originality, in his letter of the 29th March 1842 (afterwards privately printed), he endeavours to impeach mine; and, describing what passed on the 9th August, in the words already quoted in Extract Second, he adds,—

EXTRACT NINTH.—*From Professor Agassiz to Professor Forbes.*

"Je suis certain d'avoir ajouté que M. Guyot les avait vues la même année (1838), à une profoundeur notable sur le Glacier du Gries."

To prove the negative fact that M. Agassiz did not cite M. Guyot upon the occasion, I can only state (1.), that neither Mr Heath nor myself recollect his name to have been mentioned, *although* we perfectly collected M. Agassiz' meaning as to his *having observed* the linear arrangement of the sand on the surface. (2.), That had the occurrence of this structure to any depth been a recognised fact subsisting previously in the mind of M. Agassiz, whether from his own observations or those of another, Mr Heath and I would not have spent the whole afternoon in what then seemed to Mr Heath "the very superfluous endeavour to make out whether it was superficial or not." (3.), What seems decisive in the matter, M. Agassiz claimed the observation *as his own* in the letter to Humboldt, written in October; nor does he appear to have made any allusion to M. Guyot in his communication on the same subject to the Société de Physique at Geneva, which occasioned M. Guyot to mention his prior observation.

Between M. Guyot and myself there remains nothing to explain. That gentleman has never contested the originality of my observation, and I have never pretended to doubt the reality of his, which, far from being made known to the world by the publication of the proceedings at Porrentruy, seems to have slipt entirely from the memory of the persons present (including, I am informed, MM. Studer and Agassiz), whilst every written proof of it remained in manuscript. Accordingly, so soon as I had satisfactory evidence of the nature of M. Guyot's communication, I hastened to write to him, and assure him that I admitted his observation to be identical with mine. This I did in the following terms:—

EXTRACT TENTH.—*From Professor Forbes to Professor Guyot of Neufchâtel.*

"EDINBURGH, 28*th April* 1842.

"My Dear Sir,—In a printed letter which M. Agassiz has forwarded to me, I find a memorandum (printed for the first time) from your manuscript, containing an account of the structure of the Glacier of the Gries, observed in 1838, and stated to have been read at a meeting of Naturalists at Porrentruy, in that year.

"I have no hesitation in saying, that that note describes clearly a structure similar to that which I observed and pointed out to M. Agassiz and Mr Heath, on the Glacier of the Aar, on the 9th of August last.

"Whilst, then, I am most ready to do you full justice in respect to the originality and clearness of your observation, you will, I doubt not, as freely admit, that not having the pleasure of your acquaintance at the time of my observing and ascertaining the existence and modifications of *this structure on the Aar Glacier*, and never having heard, to the best of my recollection, during the course of my stay in Switzerland, of your having made such an observation, I could not in any respect have borrowed it from you. As no printed record of your communication then existed, I could not, of course, have learned of it from books. You will also, I doubt not, candidly admit, that your having failed to publish your observation in any, even the most abridged abstract,—your having omitted to press it as a fact important in the theory of glaciers upon any of your Swiss friends, and especially on M. Agassiz, who was writing a book on the subject, shews that the observation had not excited either on your part or that of your auditors at Porrentruy, any very lively interest. The fact itself would probably have been soon lost to science, if it had not been revived last summer by re-discovery, and by a strong indication of its generality, and importance in the theories now agitated.

* * * *

"Every one in the slightest degree conversant with questions of this kind will see, on reading M. Agassiz' letter, that your observations communicated three years before at a provincial meeting, not published even in the vaguest form in the minutes of the proceedings, nor alluded to in their writings by any one of the contemporary authors who are stated to have been present, leave my claim to have made the observation independently, and first insisted on its importance and generality, quite unimpeached.

* * * *

"My firm belief is, that M. Agassiz had totally forgotten this passage in the verbal proceedings at Porrentruy. I believe him to be incapable of the sustained duplicity of affected ignorance and surprise when I first pointed out the fact to his notice on the 9th of August. I believe his present newly displayed zeal for your originality in this matter to be occasioned solely by finding it impracticable to maintain the charge against me of plagiarism and ingratitude towards *himself*, which he at first alone urged.

"The dilemma in which M. Agassiz has placed himself appears to be this:—

"Either he was acquainted with this structure of ice on the 9th August, or he was not.

"If he was not acquainted with it, he learned it from me; for he has never attempted to maintain that he shewed it to me.

"If he was acquainted with it, he learned it from you. And if he learned it from either of us, how does he claim it as his own in the letter to Humboldt, and in one other private letter at least, not yet published? I am, my Dear Sir, yours very truly, James D. Forbes."

"Professor Guyot."

There are few sciences which have not offered parallel cases of insulated observations which lie dormant for many years, before, by being generalized and made units of a class of facts, *they form the* basis of theoretical induction. This is what I claim to have done:—to have re-discovered M. Guyot's unpublished and all but forgotten fact;—to have generalized it so as to shew that it was common to most if not all glaciers;—to have explained the law of its occurrence in one glacier (the lamellar surfaces of the glacier of the Rhone);—and to have applied it to account for two appearances formerly ascribed to other and imaginary causes, the distribution of sand on the surface of the ice, and the supposed stratification of the terminal face of some glaciers.

I might here close my observations on the question of priority, but I will add a sentence or two, in order to avoid all cavil.

Can it be necessary to state that M. Agassiz has found a friend—M. Dubois—obliging enough to state, in April 1842, that M. Agassiz had described to a meeting at Bâle, in 1838, a structure similar to that noticed by M. Guyot, at Porrentruy, in the same year? Is it possible that discoveries in science can be made without the consciousness of those who make them? or that a discovery made in 1838 shall be wholly misrepresented by the discoverer himself in 1840 (see *Extract Third*), claimed anew for himself in 1841, and when re-claimed in the same year by two other persons, the discoverer recollects to have heard at Porrentruy the very fact which his friends assure him they heard him claim for himself at Bâle the same year? Yet such is the *newest* claim of M. Agassiz to an observation, which a discussion respecting priority of six months' duration failed to recall to his mind, but which he is *now* persuaded that he made, upon the friendly testimony of M. Dubois of Montpéreux, in the following words:—

EXTRACT ELEVENTH.—*M. Dubois' Certificate.*

"Je soussigné conjointement avec M. Arnold Escher de la Linth, Secrétaire de la Section de Géologie de la Société Helvetique des Sciences Naturelles lors de la Réunion à Bâle, certifie que dans les notes recueillées pendant la Séance du 14 Septembre 1838, il se trouve mentionné page 12 que M. Agassiz a signalé le fait de la structure lamellaire des glaciers, et qu'il en trouvait la cause dans l'accumulation des matières congélables qui se déposent à la surface du glacier. La note est accompagnée d'un dessin représentant cette structure. FREDERIC DUBOIS.

"*Péreux*, 27 *Avril* 1842."

Now, this was on the 14th September 1838. M. Guyot made his communication on the 6th September in the same year, consequently M. Agassiz could not have revisited the Glaciers in the interval. The communication at Bâle was therefore, no doubt, a repetition of the communication at Porrentruy made eight days before, and the drawing of Agassiz was probably done from memory after the drawing of Guyot. At least, I am at a loss to explain these seemingly independent communications in any other way, nor will I even put the question, whether the structure described was a *vertical* structure at all. I do not suspect M. Agassiz of the *reserve* of having made no mention at Porrentruy that the fact of Guyot had been ascertained by himself, and then of having gone immediately to claim it as original at Bâle. I apprehend rather that the Secretaries at Bâle (to whose MS. notes we are indebted alone for any knowledge of this transaction, forgotten even by the principal actor in it) had supposed, from M. Agassiz' *verbal* communication (*de vive voix*), that whilst relating what his friend M. Guyot had seen, he was really giving an account of his own observations.

I mention this as the explanation most natural and most favourable to M. Agassiz. But I would ask, if facts and theories are to be introduced *thus* into the history of science, where is the palm of discovery ever to be bestowed? Surely a man must have very little skill as an observer, and have exercised still less thought to render his observations worth recording, if he cannot recognise his own discovery when pointed out to him, but is obliged to take the authority of his friends, at the end of three years, that he ever knew it! Such evidence is barely tolerated in the case of posthumous claims. I suppose that this is the first instance of its being gravely urged during life. That I may not be imagined to have brought forward this claim more strongly than its author has done, I quote from his letter to myself.

Extract Twelfth.—*Professor Agassiz to Professor Forbes.*

"Monsieur—Je reçois la lettre suivante de M. Dubois de Montpéreux * * * dont je crois devoir vous donner copie afin de vous prouver que de mon côté j'avais aussi remarqué dès 1838, la structure lamellaire d'une partie des glaciers, alors méme que faute de plus amples détails, je n'en ai mentionnée dans mon livre que les apparences superficielles. Vous verrez par là que si vous avez pû croire avoir fait une decouverte à ce sujet, ce n'a pû être qu'en mecomprenant ce que j'ai pû vous dire[*] sur la profondeur à laquelle ces lames descendent, et qui n'avaient été remarqueés qu'à une faible profondeur avant 1841, et par moi seulement dans le voisinage des moraines.

[Here follows the Letter, and *Extract Eleventh.*]

"Jugez maintenant si j'ai dû être surpris de vos reclamations[†] et si j'étais en droit d'y repondre comme je l'ai fait. N'ayant pas l'habitude de tenir un journal régulier des moindres particularités des observations que je fais, et addressant dans nos sociétés scientifiques toutes mes communications de vive voix, ces faits ne me sont revenus à moi-même avec les circonstances accessoires que lorsque mes amis me les ont rappelés.

* * * * *

"*Neufchatel,* 28 *Avril* 1842."

After receiving the preceding letter, I gave up all thoughts of attempting to convince M. Agassiz respecting the history of this, or indeed of any, scientific question. In the course of a few months, he had entertained four different opinions respecting the authorship of the discovery in question, and still, I suppose, has some doubt as to whether he discovered it himself in 1838, or only in 1841 ; or whether he learned it from M. Guyot at Porrentruy, or from me at the glacier of the Aar.

The structure in question, which is common to every glacier in which I have looked for it, is in some so exceedingly striking, that it would seem impossible to escape notice. Such, for instance, is the case with a glacier of great beauty and extent, and which is remarkable from being almost touched by a frequented mule-road, whence the structure is admirably seen,—I mean the glacier of La Brenva, near Courmayeur. That it has not been described by any of the modern writers on glaciers, De Saussure, De Charpentier, Hugi, Agassiz, or

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[*] See *Extract Second* for M. Agassiz' own account of what he *did* tell me of his previous observations.

[†] Of course I maintain that he had no right whatever to be surprised, since it appears from the following sentence, that he was equally ignorant with myself of what he had himself done in 1838, until receiving M. Dubois' letter, dated the day before this was written :—"*Ces faits ne me sont revenus à moi-même avec les circonstances accessoires que lorsque mes amis me les out rappelés.*"

Godefroy, is certainly a most convincing proof of how long the most evident and important facts may remain *practically* unnoticed. It can hardly be doubted that it must have been casually *seen* by these intelligent persons, who have traversed such a vast extent of glacier surface; but certainly every principle of interpretation leads us to the conclusion that it was not *observed* in such a way as facts must be to enter within the pale of science, since no trace of it is to be found in any of their writings on this very subject. I have it on the authority of three eminent persons in England, France, and Switzerland,—all men of science, much travelled, and much observing,—that, upon reading my account, they recognised what they could distinctly recall having seen on the glaciers which they had visited, though they never attempted to generalize the observation, or to attach theoretical importance to it.

In like manner the older observers, whose more vague language and antiquated terms make their meaning capable of several interpretations, may very possibly have described this appearance, without its having been handed down to their successors. I have not yet seen any evidence that they have done so, but I stated last winter to the Royal Society of Edinburgh, that I should not feel the least surprise if such an anticipation were discovered. How easy it is to find meanings in undefined phrases, *after* a well-marked truth has been announced, may be judged of from the interpretation given even by a very able and candid judge, of a passage in Godefroy's *Notice sur les Glaciers*, p. 12, as referring to the present question, but which a closer examination shews has no relation to it whatever.

I cannot conclude with any observation so just, or so much to the point, as that which Professor Studer has added to the testimony, of which I have already quoted a part [*Extract Fourth*], in a letter to myself. "C'est toujours l'histoire de l'œuf de Colombo; je ne doute pas que De Saussure, De Charpentier, Agassiz et tant d'autres, parmi lesquels je me placerai moi-même, comme vous vous y êtes placé aussi, n'aient vu cette division verticale de la glace bien avant notre dernier voyage au Grimsel:—comme Newton, aura souvent vû tomber des pommes sans songer à la lune. Dans toutes les découvertes il ne *suffit pas* de voir les choses, ou bien la science ne ferait pas *des progrès* aussi lents."

On the Natural-Historical Writings of the Chinese. By M. SCHOTT.

THE Chinese, whose literary efforts have hitherto been chiefly directed to History, Geography, and Natural History, have in these departments far surpassed all the other Asiatics in completeness, accuracy, and the discrimination of objects. The simple and clear arrangement of the rich materials collected by them renders the use of their works, when we have mastered the difficulties of the language, much easier than the evident absence of a really systematic mode of treating subjects would lead us to expect. The most important of their works in which information is given on natural productions are, 1. Actual treatises on Natural History; 2. Encyclopædias and Dictionaries; 3. Narratives of Travels in foreign countries; and, 4. Geographical Treatises.

Treatises on Natural History (or rather descriptions of nature) are first of all mentioned in the annals of the dynasty *Han*; and the oldest which have reached us belong to the 5th and 6th centuries. Altogether their number is reckoned at about forty. The newest, and that which makes the greatest claim to completeness and criticism, the *Pen-ts'ao-kang-mu* of *Li-schi-tschin,* is a work of the 16th century, and has been republished frequently without alteration. The author made use of all his predecessors, gives extracts from an almost inconceivable number of other works, and finished his own in 26 years. The *Pen-ts'ao-kang-mu* is divided into 52 books. Each article of the mineral, vegetable, and animal kingdoms, contains the following paragraphs:—1. The different names which the natural object receives in China; frequently with information as to the cause of its appellations, and when exotic, with the addition of its Indian, Turkish, and other names. 2. The actual description; under which head are given the particular locality of the production, its external characters, and all its non-medical properties. These two paragraphs are, as it were, the disinterested portions of the article, and written to satisfy pure desire for knowledge. 3. The medical properties of the whole and of individual parts. 4. An elenchus of all diseases or accidents in which the production may be employed with advantage, together with indications of the mode of use (recipes). These popular medical additions are frequently of much greater extent than the descriptive paragraphs; and we here perceive, as in other departments, an impatient eagerness for practical utility. Each descriptive paragraph is a sort of examination of witnesses; all the authorities follow one another in chronological order, and the actual view or experience of Li-schi-tschin generally comes last. False statements of his predecessors are either rectified incidentally, or in a special addendum, entitled "Corrected errors." Whenever it can be historically proved in regard to an object, that China is *not* its native country, the naturalists scrupulously point this out, even when an immense time intervenes between their own epoch and that of its introduction. The author has communicated to the Berlin Academy a few articles relating to the animal and vegetable kingdoms, wholly or partially translated.

The Encyclopædias of the Chinese are exceedingly numerous, and extremely different in style and extent. The Royal Library at Berlin possesses one of the most esteemed encyclopædias, the *San-ts'ai-t'u-hoei*, the natural historical part of which contains well executed representations of selected productions of the kingdoms of nature. The descriptions themselves are generally merely abridged articles of the *Pen-ts'ao*, but sometimes with modifications and original additions. Among the dictionaries in the encyclopædia style, there is one which deserves particularly to be mentioned, the *Buleku-bitche*, or Mirror of the Mandju Language, in which the definitions of natural objects frequently amount to actual descriptions.

The Royal Library at Berlin possesses two geographical works, between the dates of which there is an interval of 700 years. The comparison of these offers much that is instructive in an ethnological and natural historical point of view, because the surface of China at the time when the first of these works appeared (about 900 years ago) was not nearly so generally cultivated nor so well peopled, and the population had by no means so uniform a type, as at present. In both, the productions are noticed topographically, according to the political division of China as it stood at the different periods; but in comparing them we must assign the old districts to the present ones or their parts. This labour is much facilitated, however, by the local-historical sections of the geography, which always tell us how the district referred to was named under the different dynasties, or to what larger division it belonged. (*Bericht über die Verhandlungen der Königl. Preuss. Akademie der Wissenchaften zu Berlin*, 1842. p. 167.)

The Origin and History of the Red Race according to Mr Bradford.

The facts adduced in the course of the author's investigation tend, he conceives, to support the following conclusions:—

I. That the three great groups of monumental antiquities in the United States, New Spain, and South America, in their style and character, present indications of having proceeded from branches of the same human family.

II. That these nations were a rich, populous, civilized, and agricultural people; constructed extensive cities, roads, aqueducts, fortifications, and temples; were skilled in the arts of pottery, metallurgy, and sculpture; had attained an accurate knowledge of the science of astronomy; were possessed of a national religion, subjected to a salutary control of a definite system of laws, and were associated under regular forms of government.

III. That from the uniformity of their physical appearance; from the possession of relics of the art of hieroglyphic painting; from universal analogies in their language, religion, traditions, and methods of interring the dead; and from the general prevalence of certain arbitrary customs, nearly all the aborigines appear to be of the same descent and origin; and that the barbarous tribes are the broken, scattered, and degraded remnants of a society originally more enlightened and cultivated.

IV. That two distinct ages may be pointed out in the history of the civilized nations—the first and most ancient, subsisting for a long and indeterminate period in unbroken tranquillity, and marked towards its close by the signs of social decadence; the second, distinguished by national changes, the inroads of barbarous or semi-civilized tribes, the extinction or subjugation of the old and the foundation of new and more extensive empires; and,

V. That the first seats of civilization were in central America, whence population was diffused through both continents, from Cape Horn to the Arctic Ocean.

In relation to the question of their origin, it appears—

I. That the Red race, under various modifications, may be traced physically into Etruria, Egypt, Madagascar, Ancient Scythia, Mongolia, China, Hindoostan, Malaya, Polynesia, and America, and was a primitive and cultivated branch of the human family; and,

II. That the American aborigines are more or less connected with these several countries, by striking analogies in their arts,—their customs and traditions,—their hieroglyphical painting,—their architecture and temple-building,—their astronomical systems, and their superstitions, religion, and theocratical governments. It has long been a favourite theory, to trace the aborigines to a Tartar or Mongol migration from Siberia, by Behring's Straits. But the Mexicans and Peruvians resemble the cultivated nations of Oriental Asia, even more closely than do the ruder tribes, the Siberian nomades; in fact, they are *all* of the same race, and, both in Asia and America, a decline into barbarism has produced analogous developments, which, in connection with the relics of their ancient religion and customs, nearly assimilate the savages of both continents. It is not to be denied, that there are some tribes in North America, which may have proceeded in modern times from Siberia,—for example,—the Chippewyans,* and perhaps the Sioux, the Osages, Pawnees,† and some of the North-Western nations;‡ but even in relation to these, the proof depends mainly upon vague and uncertain traditions. But to suppose that the Mexicans, the Toltecs, the Chiapanese, the Mayas, and the Peruvians, were the descendants of such degraded and savage hordes as occupy north-eastern Asia; or that they wandered from more southern Asiatic countries, through the cold and inhospitable regions of the north, without leaving any vestige of civilization on their way, appears equally contrary to experience and philosophy. The ancient monuments in Siberia are situated to the west and to the south, those of America are limited in their extent on the north-west; and, in spite of the facility of communication afforded by the contiguity of the two continents in that direction, these facts would seem to be decisive of the question. On the other hand, the evidence of an early knowledge of the compass in China, of the great maritime skill of the Malays, and of their navigation, in remote ages, of the Asiatic seas, the facts stated in relation to the peopling of islands by the accidental drifting of canoes, and more than all, the actual proof of the distribution of population over the numerous and distant islands of the great Pacific, from Asia to Easter Island, render it unnecessary to resort to the violent hypothesis of a northern route. What greater obstacles were there, to impede a passage from Easter Island to the American coast, than attended a migration to Easter Island? Indeed, this island itself appears to have been successively occupied by different families; and its pyramidical edifices, and its colossal obelisks and statues, are closely analogous to the American monuments.

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* Mackenzie's Journal, pp. 387, 113.

† Pike's Expedition, part i. p. 63; part ii. p. 9, 14.

‡ *Sauer*, pp. 160, 177. Coxe, pp. 151, 257.

When and by whom was America peopled? This interesting question, if it shall ever be solved, of course can be answered only in a general manner. The character of American civilization is not wholly indigenous. Its mutual diversities are no more than might naturally arise, when nations of the same stock are separated; its uniformities are great and striking, and exhibit, in common, an astonishing resemblance to many of the features of the *most ancient types* of civilization in the Eastern hemisphere. The monuments of these nations were temples and palaces; their temples were pyramids; their traditions were interwoven with cosmogonical fables, which still retained relics of primitive history; and their religion was sublime and just in many of its original doctrines, though debased in their superstitious abuse and corruption. In all this there is nothing modern,—nothing recent; these features are not strictly Hindoo, Egyptian, or Chinese, though they approximate the aboriginal civilization to that of each of these nations. The origin of this resemblance is to be traced back to the *earliest ages,* when these great nations first separated, and carried into Egypt, Hindoostan, China, and America, the same religion, arts, customs, and institutions, to be variously modified under the influence of diverse causes. The great diversity of American languages, the few analogies they present to those of the Old World; the absence of the use of iron; certain peculiarities in their astronomical systems; and some of their own traditions which have preserved the memory of the great events of ancient sacred history, and attribute the colonization of the continent to one of those tribes who were present at the dispersion of mankind, all tend to support this position. The Red race, then, appears to be a *primitive branch of the human family,* to have existed in many portions of the globe, distinguished for early civilization; and to have penetrated at a very ancient period into America. The American family does not appear to be derived from any nation *now existing;* but it is assimilated by numerous analogies to the Etrurians, Egyptians, Mongols, Chinese, and Hindoos; it is *most closely* related to the Malays and Polynesians; and the conjecture possessing perhaps the highest degree of probability, is that which maintains its origin from Asia, through the Indian Archipelago.

The most remarkable peculiarity in the institutions of all these nations, is their religious character. Laws, government, the arts and sciences, and the whole routine of private and public affairs, were under the direction of the priesthood. Thence several consequences flowed,—the preservation from a rapid decline into barbarism, so long as religion retained its supremacy,—the utter absence of progression and improvement,—and the stereotype character of the whole system of society. The sciences were occult; long religious probations were necessary before their principles were taught, and thus no generation possessed an advantage over the preceding one. Knowledge and civilization were not animate and instinct with natural warmth and vigour, but were embalmed, and like a shrivelled mummy, presented the mere outward form with none of the vitality of existence. From this continued religious subjection originated also, that unchangeableness, that fixed and immutable character, which distinguished all these nations, and which is a marked and prominent trait of the savage Indian. An inflexibility which adheres tenaciously to old forms and customs, and despises change; which may be overpowered, but never yields; and which, in view of the dreary impending fate of the aborigines, possess an air of melancholy grandeur; for, as one of those coming events which "cast their shadows before," the absolute extinction of this ancient race seems to be rapidly and irresistibly approaching. Upon this continent, the pure types of the new and the old era of civilization have met and encountered each other. The family presenting the one, having occupied this vast region for countless ages undisturbed by the approach of other and modern races, had been allowed the amplest scope for development, and yet, at the discovery, the greater portion of the continent was inhabited by savage hordes; within the United States, the barbarous tribes appear to have been greatly depopulated, and the ancient cultivated nations to have become extinct; even in Mexico and Peru, the civilization of the first ages seems to have surpassed that of later times, and society generally was in a state of decadence. The old system,—its moral and social elements,—its capacity for self-improvement,—had thus been fairly tried and tested; and the time had arrived when a new race, and the Christian religion, were appointed to take possession of the soil.—(From an interesting work lately published *On the Origin and History of the Red Race, by Alexander W. Bradford.*)

Mean Results of the Thermometer, and the Quantity of Rain, for 1841, *at Alford in Aberdeenshire—about Lat.* 57° 13′ *N.;* 420 *feet above the level of the sea, and* 26 *miles inland from the sea at Aberdeen. Also, the number of fair days, and of days on which rain or snow fell, more or less.* By the Rev. JAMES FARQUHARSON, LL.D., F.R.S. Communicated by the Author.

THE Thermometer was registered at 8¾h. A.M., and 8h. P.M., and the daily extremes, indicated by self-registering Thermometers, were registered at the latter hour.

1841.	THERMOMETER.						RAIN.		
	Mean of Morn.	Mean of Even.	Mean of Morn. and Even.	Mean of daily highest and lowest.	Highest in the month.	Lowest in the month.	Rain in Inches.	Number of fair days.	Number of rainy days.
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.			
Jan.......	30.193	30.7	30.446	31.5	45	−7	2.8	12	19
Feb......	35.39	35.82	35.6	36.19	51	25	2.3	13	15
March,	43.38	43.06	43.22	43.82	60	32	1.1	18	13
April, ...	44.1	42.93	43.51	43.35	60	30	1.0	13	17
May, ...	52.97	51.77	52.37	51.13	72	31	2.35	19	12
June, ...	52.8	51.9	52.35	52.42	68	36	0.8	15	15
July, ...	55.	53.8	54.4	54.14	70	44	4.55	12	19
August,	56.22	55.52	55.87	55.41	68	40	4.2	16	15
Sept. ...	53.43	53.03	53.23	53.38	66	38	3.2	16	14
October,	41.9	42.58	42.24	42.22	54	25	5.5	8	23
Nov......	35.63	37.53	36.58	37.18	56	11	1.8	16	14
Dec......	35.74	37.19	36.46	37.31	48	19	4.1	16	15
Means,	44.729	44.65	44.689 Mean of year.	44.92			33.7 Rain of the year.	174	191

	Deg.
Mean temperature from April to September, both inclusive,	51.95
Mean Temperature of July, August, September. . .	54.5

	Deg.	Rain in Inches.	
Mean temperature of 1833, ...	44.573		
... ... 1834, ...	47.99		
... ... 1835, ...	45.93	37.7	
... ... 1836, ...	44.713	45.55	
... ... 1837, ...	44.73	32.05	
... ... 1838, ...	43.0933	41.25	
.. .. 1839, ...	44.521	36.3	
... ... 1840, ...	45.156	34.575	
... ... 1841, ...	44.689	33.7	
Mean temperature of nine years,	45.0439	37.707	Mean of seven years.

The adoption of Sir David Brewster's suggestion—to make each of the daily observations half-an-hour earlier than the hours at which the mean temperature came out at Leith, has brought the means of the morning and evening hours within one thirteenth of a degree of each other; so that, as far as one year's observations can determine the matter, his calculation of the hours at which the mean temperature occurs at Alford seems very correct.

The year 1841 has been, as a whole, unkindly to the productions of the earth. This has been owing to the unfavourable distribution of the heat and moisture to the different months, rather than to any mean deficiency of the former or excess of the latter. The dry months of March and April were highly favourable for cultivating the ground and depositing the seed in it; but unseasonably dry weather, from about the 10th of May to the 4th of July, prevented the grain crops from stocking, and making the usual progress; and caused the hay crop to be very deficient. From the 4th, the month of July proved cold and wet, and unusually gloomy—to such a degree, indeed, that a part of the oats failed in the flowering process; a thing very uncommon in that hardy grain. August proved favourable, especially after the 13th; and as September, till the 25th, was of the average temperature for the season, the grain crops, although deficient in quantity, filled and ripened tolerably well. From that latter date repeated heavy rains, without intermediate windy days, as occurred in the harvest of 1840, rendered the labours of the harvest extremely difficult, and the grain suffered considerable damage by sprouting in the sheaf. The weather did not steadily clear up till November, when the latest cut grain was ultimately secured in greater safety than what was cut earlier. Happily, amidst these unfavourable circumstances, and although the ripening of the grain was somewhat late, no frost intervened to damage it while abroad in the fields; and neither the deficiency of the produce, nor the injury inflicted by the wet harvest, are of such magnitude as to excite much alarm.

The unusual number of the days on which rain has fallen, and the gloominess of the later summer and of the autumnal months, have rendered the year a very uncomfortable one to the human feelings.

Abstract of Meteorological Observations for 1841, *made at Applegarth Manse, Dumfriesshire. By the*

Long. 3° 12′ W. Lat. 55° 13′ N.; Height above the Sea, 180 feet; Distance from the Sea, 10 miles; Rain-Gauge, 5 feet from the Ground.
The observations made at 9 A.M. and 9 P.M.

BAROMETER.

1841. Months.	Atmospheric Pressure, Morning.	Red. to 32° Fahr., and corrected to sea-level.	Atmospheric Pressure, Evening.	Reduced and Corrected.	Mean of Morning and Evening.	Reduced and Corrected.	Mean Daily Range.	Mean Nightly Range.	Mean Range of 24 hours.	Monthly Extremes. Highest.	Monthly Extremes. Lowest.	Monthly Extremes. Greatest range in 24 hours.	Monthly Extremes. Least range in 24 hours.
January,	29.635	29.788	29.647	29.770	29.641	29.779	0.142	0.134	0.276	30.200	28.900	0.940	0.020
February,	29.713	29.844	29.695	29.815	29.704	29.829	0.111	0.100	0.211	30.420	28.940	0.540	0.060
March,	29.665	29.776	29.663	29.759	29.664	29.767	0.143	0.128	0.271	30.330	29.010	0.800	0.050
April,	29.681	29.801	29.694	29.814	29.687	29.807	0.106	0.098	0.204	30.170	29.280	0.410	0.040
May,	29.700	29.802	29.711	29.816	29.705	29.809	0.109	0.110	0.219	30.250	29.830	0.680	0.020
June,	29.825	29.921	29.818	29.927	29.821	29.924	0.080	0.072	0.152	30.150	29.420	0.400	0.000
July,	29.681	29.777	29.682	29.777	29.681	29.777	0.106	0.063	0.169	30.060	29.270	0.560	0.020
August,	29.680	29.775	29.716	29.812	29.698	29.793	0.086	0.096	0.182	30.070	29.340	0.340	0.010
September,	29.594	29.690	29.594	29.690	29.594	29.690	0.066	0.086	0.152	30.090	28.700	0.400	0.020
October,	29.417	29.640	29.407	29.631	29.412	29.635	0.173	0.169	0.342	30.020	28.730	0.890	0.040
November,	29.866	30.013	29.535	29.682	29.700	29.847	0.098	0.121	0.219	30.290	28.440	0.620	0.040
December,	29.140	29.293	29.422	29.581	29.281	29.437	0.110	0.180	0.290	29.950	28.780	0.890	0.030
Means,	29.633	29.760	29.632	29.752	29.632	29.757	0.110	0.113	0.223	30.166	28.970	0.622	0.029
1840,	29.719	29.829	29.706	29.806	29.713	29.817	0.102	0.108	0.210				

THERMOMETER.

Months.	Mean of greatest Heat.	Mean of greatest Cold.	Mean Temp. of Morning.	Mean Temp of Evening.	Mean of Extremes.	Mean of Morning and Evening.	Mean of both.	Mean range of 24 hours.	Monthly Extremes. Highest.	Monthly Extremes. Lowest.	Monthly Extremes. Greatest range in 24 hours.	Monthly Extremes. Least range in 24 hours.	Highest at 9 A.M.	Lowest at 9 A.M.	Highest at 9 P.M.	Lowest at 9 P.M.	Temp. of Spring Water.
Jan.	37.00	26.70	30.20	32.10	31.80	31.10	31.45	10.10	48.00	1.00	23.50	1.50	41.50	2.00	44.00	11.00	42.00
Feb.	40.50	32.20	36.10	36.50	36.80	36.30	36.55	6.82	48.50	26.50	15.00	1.00	44.00	27.00	46.00	28.00	42.60
March	50.20	39.20	43.40	43.50	44.70	43.45	44.07	11.04	62.00	26.50	21.00	2.00	50.50	31.00	49.00	33.50	45.60
April,	51.50	37.60	45.40	43.10	44.55	44.25	44.40	14.10	64.00	28.50	23.00	6.50	55.00	40.00	52.50	36.00	51.00
May,	60.20	44.40	53.30	50.90	52.30	52.10	52.20	15.70	78.00	32.50	27.00	6.00	68.00	44.00	61.00	43.50	49.33
June,	61.00	47.40	55.10	52.70	54.20	53.90	54.05	14.30	76.00	38.00	26.00	6.00	65.00	48.50	57.00	47.00	50.80
July,	61.00	49.10	55.30	54.00	55.05	53.65	54.35	11.00	67.00	55.00	18.00	7.50	63.00	51.50	59.00	50.00	50.00
August,	62.10	50.50	55.30	56.00	56.30	55.60	55.90	11.50	71.00	41.50	20.50	4.30	61.50	51.50	61.00	51.00	53.60
Sept.	62.20	48.30	55.30	55.10	55.20	55.20	55.20	13.70	81.00	33.50	26.00	0.50	69.00	47.00	68.00	44.50	50.80
October.	51.70	40.60	45.00	45.70	46.15	45.35	45.75	7.80	62.50	30.00	22.00	6.00	52.50	34.50	54.50	36.50	48.30
Nov.	44.00	34.30	37.00	39.40	39.15	38.20	38.67	9.80	53.00	18.00	17.00	1.50	53.00	20.00	52.50	25.00	45.60
Dec.	43.80	34.40	37.50	40.00	39.10	38.70	38.90	9.54	51.00	22.00	17.00	3.50	48.00	23.00	49.50	25.00	41.60
Means,	52.10	40.39	45.74	45.78	46.27	45.65	45.95	11.28									
1840,	51.82	40.47	46.40	45.52	46.14	45.96	46.03	11.71									

Dr Dunbar's *Meteorological Observations for* 1841.—(Continued.)

WINDS—THEIR DIRECTION AND FORCE, AND WEATHER, STATED IN THE NUMBER OF DAYS IN WHICH EACH PREVAILED.

MONTHS.	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.N.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	Calm.	Mod.	Brisk.	Strong Breeze.	Storm.	Sun Shone.	Rain Fell.	Snow.	Frost.	Thunder.	Rain in Inches.
January,	2	—	5½	—	2	3½	1½	—	—	—	4	1	4	2½	3	2	8	8	3	17	1	25	10	8	16	—	2.90
February,	1	—	8	2	2	1	4	—	4	—	2	—	1	—	1	2	6	11	4	7	—	19	8	6	11	—	3.78
March,	—	—	—	—	1	—	4	1	4	1	13	2	3	—	1	1	11	8	5	7	—	23	18	1	2	—	3.37
April,	½	—	3	—	2	—	4	2	2	½	7	1	6	—	1	1	6	14	4	6	—	27	20	H 2	2	2	1.93
May,	—	—	1	—	3	—	4	—	4	1	14	3	1	—	—	—	12	10	4	5	—	29	15	H 1	—	2	3.24
June,	2	—	4	1	3½	—	2	2	3½	1	4	—	4½	1	1½	—	7	9	2	12	—	28	11	H 1	—	4	1.98
July,	—	1½	1½	—	3	1	1½	—	½	1½	4½	—	7	2	6	1	10½	13	1½	6	—	27	20	—	—	2	3.32
August,	—	—	—	—	3	—	—	—	2	3	9½	—	6	3	3½	1	8	9	11	3	—	24	22	—	—	3	5.01
September,	—	1	1	1	7	4	4	1	4	2	2	1	—	1	1	—	9	9	4	8	—	26	15	—	2	5	3.39
October,	2	2	6	1	1½	1	2	2	1	2	4½	—	2	1	1½	1½	3	9	10	8	1	26	19	—	4	—	5.10
November,	1	1	5	2	6	—	1½	—	2	1	3½	1	3	1	1	1	9	9	6	6	—	20	17	2	9	—	2.37
December,	½	½	1	—	2	—	1½	—	—	4	11½	1½	—	—	5½	3	9	9	—	6	1	21	21	1	10	—	3.41
Total,	9	6	36	7	36	10½	30	8	27	17	79½	10½	37½	11½	26	13½	98½	118	54½	85	3	295	196	S 18 H 4	56	18	39.80
1840,	14½	5½	31½	17	34½	12	17	8	42	12	70	17	36½	9	30½	9	135½	109½	55	60	4	310	172	10	60	12	30.26

REMARKS.

The maximum of atmospherical pressure was on the 1st of February, when the Mercury stood at 30.470 inches. In 1840, the maximum was, on the 8th of March, 30.590 inches. The minimum of 1841, was 28.440, on the 30th November. Of 1840, 28.410, on the 24th January. The oscillations of the Barometer were in a slight degree more frequent in 1841 than in 1840, the former mean range in 24 hours, being 0.223 of an inch, the latter 0.210. The whole range of the year, 2.030 inches. The maximum of temperature was on the 12th of September, viz. 81 degrees. In 1840, it was 75 degrees. The minimum of 1841, was on the 7th of January, viz. 1.00. Of 1840, 19.50 degrees. The mean of extremes exceeds the mean of Mornings and Evenings by six-tenths of a degree. The mean temperature of 1841 is nearly one-tenth of a degree lower than that of 1840, and nearly three-tenths below the average of the last 20 years.

The Rain in 1841 exceeded that of 1840 by no less than 9½ inches, and the average of the last 15 years by 4½ inches. The driest month in the year was April, the wettest, October. The prevalence of South-west winds in this part of the kingdom was very marked, it having blown from that quarter of the compass, 79½ days. The number of days marked calm, is less by about 40 than in the preceding year.

Proceedings of the Royal Society of Edinburgh.

(Continued from vol. xxxiii. p. 197.)

January 17. 1842.—Dr ABERCROMBIE, V. P. in the Chair.

1. On the Identity of the Animal Matters which form the Basis of the Animal Fluids and Solids. By James Stark, M.D., F.R.C. Phys.
2. On the Parasitic Fungi found growing on the Bodies of Living Animals. By John Hughes Bennett, M.D. Communicated by Dr Graham. Part I.

February 7.—Sir T. M. BRISBANE, Bart., President in the Chair.

1. On the Parasitic Fungi found growing on the Bodies of Living Animals. By John Hughes Bennett, M D. Communicated by Dr Graham. Part II.

2. On the Action of Water on Lead. By Dr Christison.

The author, after briefly stating the results of his Experimental Inquiries, published on this subject in 1829, proceeded to describe two instances which had recently come under his notice, illustrative of the solvent action of certain terrestrial waters on lead, and of the danger of using this metal for conducting water in pipes, unless with a due regard to the circumstances which promote or prevent its corroding property. In one instance, the water of a spring, conveyed in a lead-pipe from a distance of three quarters of a mile, was found to act so powerfully on the lead, that in a short time the cistern in which the water was collected became covered with loose carbonate of lead, and the metal could easily be detected in the state of oxide dissolved in the water. In this case, the action was found to depend on the spring being of extraordinary purity, its total saline ingredients being only a 22,000th part. In the other instance, water conveyed half a mile in a lead pipe, was impregnated exactly in the same way, and with the very same phenomena,—but with the additional circumstance, that, in consequence of the impregnation not having been detected in time, as in the previous case, the disease, *Colica pictonum*, broke out in the house supplied with the water. In this case, the water was by no means pure, as it was found to contain no less than a 4,500th part of saline matter. But there was scarcely any other salt present except muriates, which the author had ascertained in his former researches not to prevent the action of water on lead, unless present in much larger quantity.

He next proceeded to explain in what manner the action of the water was put an end to in both these cases. In similar instances, the only remedy formerly thought of was substitution of iron-pipes. In the former of the two cases which fell under his notice, the water was left at rest in the pipe for four months, till a firm crust of mixed carbonate and sulphate of lead had crystallized on the lead; after which no farther action took place. In the latter instance, the same end was attained by keeping the pipe full of a solution of phosphate of soda, consisting of 27,000th of the salt.

The author appended an analysis of the compound formed by the action of distilled water on lead. Guyton-Morveau and others considered it a hydrated oxide; the author himself, in 1829, thought it a neutral carbonate; and, in 1834, Captain Yorke first considered it a hydrated oxide, and eventually concluded from his analyses, that it is an irregular mixture of hydrated oxide and carbonate of lead. The author finds that the product is a hydrated oxide, when the action goes on without the access of carbonic acid; but that, when the action proceeds in the usual way, under exposure to the atmosphere, the product is a crystalline body, of which the primitive form seems to be the regular octahedron, and which is composed of two equivalents of neutral carbonate, united with one equivalent of hydrated oxide ($2\,PbO\,CO^2 + PbO\,Aq$).

He then stated the following to be the general conclusions to be drawn in a practical point of view, from his present and previous inquiries as to the use of lead for conveying water:—

1. Lead-pipes ought not to be used for the purpose of conveying water, at least where the distance is considerable, without a careful chemical examination of the water to be transmitted.

2. The risk of a dangerous impregnation with lead is greatest in the instance of the purest waters.

3. Water, which tarnishes polished lead, when left at rest upon it in a glass vessel for a few hours, cannot be safely transmitted through lead-pipes without certain precautions.

4. Water, which contains less than about an 8000th of salts in solution, cannot be safely conducted in lead-pipes, without certain precautions.

5. Even this proportion will prove insufficient to prevent corrosion, unless a considerable part of the saline matter consist of carbonates and sulphates, especially the former.

6. So large a proportion as a 4000th, probably even a considerably larger proportion, will be insufficient, if the salts in solution be in a great measure muriates.

7. In all cases, even though the composition of the water seems to bring it within the conditions of safety, now stated, an attentive examination should be made of the water, after it has been running for a few days through the pipes. For it is not improbable, that other circumstances, besides those hitherto ascertained, may modify the preventive influence of the neutral salts.

8. When the water is judged to be of a kind which is likely to attack lead-pipes, or when it actually flows through them impregnated with lead, a remedy may be found, either in leaving the pipes full of the water, and at rest for three or four months, or by substituting for the water a weak solution of phosphate of soda, in the proportion of about a 25,000th part.

February 22.—The Right Hon. Lord GREENOCK, V. P., in the Chair.

1. On the Necessity of the Sense of Muscular Action to the full Exercise of the Organs of the Senses. By Sir Charles Bell, K.H.

March 7.—Sir T. M. BRISBANE, Bart., President, in the Chair.

1. On the most recent Disturbance of the Crust of the Earth, in respect to its suggesting a Hypothesis to account for the Origin of Glaciers, By Sir George Mackenzie, Bart.

(Published in this Journal, vol. xxxiii. p. 1.)

2. Geological Notes on the Alps of Dauphiné. By Professor Forbes.

The district proposed to be described, in so far as it was studied by the author in two journeys in 1839 and 1841, is an out-lyer or appendage to the main Alpine chain, which occupies a considerable portion of the old province of Dauphiné, and the modern departments of the *Hautes Alpes* and *Isére.* It is bounded, roughly, by the rivers Arc and Isére on the north, and by the Durance and the Drac in other directions. Its nucleus is essentially granitic, against which sedimentary deposits of limestone, of different ages, and especially of lias and chalk, repose in highly elevated or contorted strata; and it not unfrequently happens, that the dislocation of strata has been so great, that the gneiss or granite rocks are superimposed upon the secondary formations.

The granitic mountains of Oisans, which are amongst the highest of the second order of European chains, attain a greater elevation at their culminating point, the Mont Pelvoux, than any of the Alps between Mont Blanc and the Mediterranean. Even Mont Iseran and Monte Viso are surpassed in height by this summit, which measures 13,468 English feet. The ravines by which the chain is intersected have a corresponding depth and ruggedness, so that the *cols*, or passages from one valley to another, are generally covered with perpetual ice and snow, and present, besides, more continuous and inaccessible precipices than are common in any part of Switzerland. The author shortly described several journeys made through the central part of this district, in which it became necessary to cross *cols* of above 10,000 feet in height, from whence alone an intimate knowledge of the structure of these mountains can be obtained.

Guided by the interesting memoir of M. Elie de Beaumont, on the geology of the *Montagnes d'Oisans*, and by the admirable map of Bourcet, he was enabled, in a great many particulars, to verify the observations of the first named distinguished geologist, especially as refers to the phenomena visible at the contract of the calcareous and granitic rocks, which left no doubt on the author's mind that the superposition of the latter to the former is undeniably true. No more can it be doubted, that, as M. E. de Beaumont affirms, we have here evidence of the extensive elevation of previously deposited sedimentary rocks, probably by the appearance from below of the granite itself. Professor Forbes feels some hesitation in admitting, with M. de Beaumont, the *crateriform* nature of this elevation, as indicated by a *quâ-quâ-versal* dip of the stratified rocks round a central point in the neighbourhood of the Mont Pelvoux, and by the radiation of the vallies from that centre. He considers that the observations of the great French geologist, when analyzed, as well as his own, rather point to an anti-clinal axis passing through the point in question, and prolonged in a NN.W. and SS.E. direction; accompanied, however, with various minor lines or centres of dislocation, especially that which elevated the mountain of Grande Rousse to the northward, of which the geology has been ably described by M. Dausse. The interference of this elevation with the previous one (roughly parallel to the torrent of the Veneau), probably produced the excessive disturbance of the strata of lias near La Grave, which have been jostled between the two granite masses.

These views are supported, partly by a consideration of the external *contour* of the group, and partly by direct observations of the bearing and dip of the strata.

March 24.—Dr Abercrombie, V.P. in the Chair.

1. On a New Species of British Grass of the genus Holcus, and Observations on some of the more closely allied species of Grasses found in the Neighbourhood of Edinburgh. By Richard Parnell, M.D., F.R.S.E.

2. On the Ultimate Secreting Structure of Animals. By John Goodsir, Esq. Communicated by Professor Syme.

After referring to the labours of those anatomists who had verified Malpighi's doctrine of the follicular nature of gland ducts, the author alluded to Purkinje's hypothesis of the secreting function of the nucleated corpuscules of these organs. In a rapid sketch of the results of inquiries since the appearance of Müller's work "De Penitiore Structura Glandularum," and more particularly of the observations of Henle and others on the closed vesicles which are situated at the extremities of certain ducts, Mr Goodsir stated, that no anatomist had hitherto "proved that secretion takes place within the primitive nucleated cell itself, or had pointed out the intimate nature of the changes which go on in a secreting organ during the performance of its function."

Numerous examples were now given of secretions detected in the cavities of nucleated cells of various glands and secreting surfaces. Among those secretions were the ink of the Cephalopoda, and the purple of *Janthina* and *Aplysia*, bile in an extensive series of animals, urine in the mollusk, milk, &c.

The wall is believed by the author to be the part of the cell engaged in the process of secretion. The cavity contains the secreted substance, and the nucleus is the reproductive organ of the cell. A primitive cell engaged in secretion is denominated by the author a primary secreting cell; and each cell of this kind is endowed with its own peculiar property, according to the organ in which it is situated. The discovery of the secreting agency of the primitive cell does not remove the principal mystery in which the function has always been involved; but the general fact that the primitive cell is the ultimate secreting structure is of great value in physiology, inasmuch as it connects secretion with growth as phenomena regulated by the same laws; and explains one of the greatest difficulties in the science, viz. why a secretion flows from a free surface only of a secreting membrane,—*the secretion exists only on the free surface enclosed in the ripe cells which constitute that surface.*

The author then proceeded to the consideration of the origin, the development, and the disappearance of the primary secreting cell—a subject which necessarily involved the description of the various minute arrangements of glands and other secreting organs. After describing the changes which occur in the testicle of *Squalus cornubicus*, when the organ is in a state of functional activity, and in the liver of *Carcinus mœnas*, it was stated that these were selected as examples of two orders of glands denominated by the author vesicular and follicular.

The changes which occur in the first order consist in the formation and disappearance of closed vesicles or acini.

Each acinus might be, first, a single cell, denominated by the author the primary or *germinal* cell; or, secondly, of two or more cells enclosed in the primary cell, and produced from its nucleus.

The enclosed cells he denominates the secondary cells of the acinus, and in the cavities of these, between their nuclei and cell-walls, the peculiar secretion of the gland is contained. The primary cell with its included group of cells, each full of secretion, is appended to the extremity or side of one of the terminal ducts, and consequently does not communicate with that duct, a diaphragm formed by a portion of the primary cell-wall stretching across the pedicle. When the secretion in the group of included cells is fully elaborated, the diaphragm dissolves or gives way, the cells burst, and the secretion flows along the ducts, the acinus disappearing, and making room for a neighbouring acinus, which has in the mean time been advancing in a similar manner. The whole parenchyma of glands of this order is thus, according to these observations, in a constant state of change,—of development, maturity, and atrophy,—this series of changes being directly proportional to the profuseness of the secretion.

In the second order of glands, the follicular, as exemplified in the liver of *Carcinus mænas*, the germinal cell or spot, is situated at the blind extremity of the follicle, and the secreting cells, as they advance along the follicle, become distended with their peculiar secretion.

Among other general conclusions deducible from these observations, it appeared that ducts are to be considered as intercellular passages, into which the secretions formed by cells are cast.

Finally, the author inferred from the whole inquiry, 1. That secretion is a function of the nucleated cell, and takes place within it; and, 2. That growth and secretion are identical—the same process under different circumstances.

April 4.—Sir T. M. Brisbane, Bart., President, in the Chair.

1. On the Theoretical Investigation of the Absolute Intensity of Interfering Light. By Professor Kelland.

2. On the Quarantine-Classification of Substances, with a View to the Prevention of Plague. By John Davy, M.D., F.R.S., L. & E.

3. Results of Experiments on the Specific Heat of Certain Rocks. By M. Regnault of Paris. Communicated by the Secretary.

Professor Forbes observed, that, in his communication to the Royal Society on the Conductivity of Soils for Heat, on the 20th December last (see Proceedings, page 343*), he had referred to the separation of the conductivity and specific heat, which are involved in the results of the thermometric experiments on subterranean temperature. In order to eliminate the effect of specific heat, M. Regnault of Paris (well known by his experiments on this subject) undertook, at the request of M. Elie de Beaumont, to ascertain the specific heats of the soils in which the different sets of thermometers are sunk. These are communicated in a letter from M. E. de Beaumont to Professor Forbes, as follows :

	Specific Heat.
Porphyry of the Calton Hill,	0.20654
Another experiment,	0.20587
Mean,	0.20620
Sand of the Experimental Garden,	0.19432
Sandstone of Craigleith Quarry,	0.19257
Another experiment,	0.19152
Mean,	0.19205

Some correction would no doubt require to be made for the quantity of moisture contained in the rocks.

4 On the Effect of Snow in apparently increasing the Force of Solar Radiation. By Professor Forbes.

Referring to a communication made by him to the Society on the 1st February 1841 (see Proceedings, page 322), the author reminded the Society that he had then endeavoured to account for certain anomalous facts observed by Dr Richardson, connected with solar radiation in the Polar Regions, by adverting to the intense radiating effect of a covering of snow. The disappearance of this snowy covering in the month of May, the author had observed to be synchronous with the anomalous diminution of solar radiation, ascertained by a blackened thermometer, in the months of June and July, compared with the months of April and May.

Professor Forbes endeavoured to verify his conjecture, by direct experiments on the force of the sun amongst the snowy mountains of Switzerland; and it was so completely borne out, that the limited range of his instrument (Leslie's photometer) was in clear weather always outrun, when it was exposed on a snowy surface; and even when placed upon a dark rock (on the moraine of a glacier), the reflected light from the neighbouring snowy summits was so intense as to give extraordinarily high indications. Owing to the construction of the instruments, he was unable to estimate their readings correctly; but he hopes to make more accurate observations during the ensuing summer. Sir John Herschel's actinometer gave a value of the solar radiation nearly independent of its position upon snow or rock.

April 18.—The Right Hon. Lord GREENOCK, V. P. in the Chair.

1. On the Structure, Formation, and Movement of Glaciers; and the probable cause of their former extension and subsequent disappearance. By James Stark, M.D., F.R.S E.

The author endeavoured to prove, from the recorded facts stated by different writers, that the crystalline particles of which the ice of glaciers is composed, do not sensibly enlarge after being consolidated into compact ice; that the crystals have been shewn to be fully and perfectly formed in the course of a few nights in the Polar Regions; and that they have a position perpendicular to the layer of ice which they form,—their length being thus determined by the thickness of that layer.

The author next considered the different forms of stratification met with in glaciers, and stated that the greatest confusion prevailed on this point, different forms of stratification being confounded together. He therefore considers glaciers as composed of—

1. *Horizontal Strata*, or layers lying in the position in which they were first deposited, and only seen in the upper regions of the mountains. He stated that these strata were usually regarded as marking the additions which the icy mass had annually received, each layer being the accumulated snow of one year; but that, as the Meteorological Tables kept at the Hospice of the Great St Bernard shewed that from 300 to 700 inches of snow fell during the six winter months, it seemed possible that each layer marked the separate storms of snow; or, if they marked the annual accumulations, they apparently proved, what had not previously been suspected, that snow and ice waste nearly as rapidly in the upper as they do in the lower regions.

2. *Vertical and Longitudinal Strata.* The author stated, that these strata were always of great tenuity, were more or less perpendicular, but had always a direction parallel with the retaining wall or length of the glacier. Their mode of formation he attributed to

the onward movement of the glacier leaving narrow spaces intervening between the sides of the already formed icy mass and the flanks of the valley, which, being filled up with the loose and softened snow lying on the sloping flanks, was, from the falling of the temperature during the night, and from contact with the already formed icy mass, converted into a layer of solid ice. From the thinness of those layers, the author regarded them as marking the additions which had been daily made to the glacier. The author also stated that it would, in all probability, be found that, wherever pillars, pyramids, or needles of ice were met with, this structure would be found present; as the fissures which always crossed the glacier from side to side, divided it into transverse sections, which, when unequally supported below, would split into smaller fragments in the planes of their stratification, so that each fragment would necessarily assume the form of a vertical prismatic column.

3. *A combination of the Horizontal with the Vertical and Longitudinal Strata.* The author stated, that, as the mass composed of the horizontal strata of the upper regions slowly advanced to the lower ones, it received, in the manner above stated, a lateral increase, which, at the same time that it increased its breadth, probably also added to its depth. That, as the glacier continued to advance, the horizontal strata, which lay uppermost, would melt away first, so that at one point they would only be observed in the middle of the glacier, and lower down even completely disappear. He mentioned several facts which seemed to prove his position.

4. *Transverse more or less inclined Strata.* The author stated that this variety of stratification had not been recognised as a distinct form, but had been confounded with the horizontal stratification. He stated that this form would only be met with when the original structure of the glacier had been broken up and destroyed by some obstructing barrier or other cause. He instanced as the most marked example of this the terminal portion of the Rhone glacier, after it pours into the valley of the Rhone over its rocky barrier or precipice. He described the strata as being formed close to the icy mass on which the icy cataract descends, originally parallel to each other, and with a dip of 70°; but that, as new layers are formed, and the first formed layers are pushed forwards, they lose their parallelism to each other, and assume angles of dip less and less as they approach the termination of the glacier. This

change of dip and of parallelism the author attributed to the forward movement and plasticity of the mass, together with the greater amount of friction below, where the ends of the layers were in contact with the ground, and the constant deprivation of support anteriorly and below, from the continued melting of the ice at these parts, which would give the layers a constant tendency to fall forwards.

The author then proceeded to shew that fissures or crevices in glaciers could not be produced in consequence of the unequal expansion of the ice itself, nor in consequence of the expansion of the air contained within its pores; but that in every case crevices were produced in consequence of the movement of the glacier over the inclined plane on which it rested.

The author next passed to the second division of his subject, the Movement of Glaciers, and first commented on the Dilatation Theory. He endeavoured to prove that none of the phenomena observed in glaciers could be accounted for by that theory; that a glacier was not retarded in its movement though riddled with crevices; that the supposed dilatation did not alter the form of the walls of these crevices; that it did not close them at their upper extremity nor widen them out below; that it did not give rise to any convexity of the surface of the glacier; that the icy mass did not require to touch the rocky walls of the valley through which it passed; that it could move onwards for miles quite unsupported on its margins; that during a whole summer, whilst its movement was greatest, it never dilated even the few feet requisite to fill up the spaces intervening between its margin and the rocky walls of the valley; that it advanced during the heat of the day, and during winter, when it is allowed no dilatation can take place; that it was unlikely water could percolate during the course of one day through a solid mass of ice, more than 100 feet thick, especially when that ice was colder than the freezing point of water; that pools of water (in the Polar Regions) remained unfrozen for whole weeks during the summer, whilst their progressive motion was greatest. For these and other reasons, the author arrived at the conclusion, "that glaciers do not advance in consequence of a process of dilatation of their icy mass."

The author next enquired into the proofs of the truth of the sliding theory, and stated, that he had satisfied himself that every phenomenon known to occur in glaciers could be explained by it. He brought forward, as explanatory circumstances, the descent of avalanches;—the descent of trees, along the slide of Alpnach;—the fact proved by the meteorological tables kept at the Hospice of the Great St Bernard, when compared with the descent of Hugi's hut on the Aar Glacier,—that the greater the fall of snow in the upper regions during winter, the greater is the descent of the glaciers during the following summer;—and lastly, the fact that the higher the mountain range (and of course the greater the quantity of ice or snow), the lower was the level to which glaciers descend. He also endeavoured to shew that the glaciers, or icy masses, covering the mountains, and filling their vallies, at no part of their course are frozen to the soil on which they rest; and that the temperature of the soil covered with deep masses of snow or ice, was probably never below 32° Fahrenheit.

The author made a short digression here, to account for the probable cause of the former extension of glaciers, and their subsequent disappearance. He endeavoured to shew, that the scattered boulders, &c. marking the former extension of glaciers, were all over the surface of the older alluvium (diluvium of Buckland) and he hence endeavoured to ascertain at what period that alluvium was formed. After a full examination of the subject, and especially from the examination of the fossil remains found in that alluvium, he arrived at the conclusion, that the waters of the deluge were the cause of the formation of that alluvium; and he accounted for the former extension of glaciers, by the known effect of water, in the act of evaporating, producing cold, especially when acted on by a brisk wind, which was the state of the earth immediately after the deluge. The increased moisture in the atmosphere at this period, he thought, would furnish ample supplies of snow and ice for the purpose, and being first deposited on the elevated peaks, would rapidly spread over all those extended surfaces which glaciers are thought once to have covered. Their subsequent disappearance he accounted for, by supposing that the icy or snowy covering prevented the loss by radiation of the heat received by the earth's crust from the interior of the earth; since this heat, gradually accumulating below, would in time melt the icy masses at their lower extremities faster than they could be supplied from above, and thus reduce them to their present dimensions. He illustrated this view, by mentioning the fact, that the angular boulders, &c. are pretty equally scattered over all the extended surfaces which glaciers are thought formerly to have covered, but are rarely seen to form the dykes or moraines seen at the terminations of glaciers at present in existence; this fact apparently proving that they must have commenced their decay very shortly after their formation.

The author stated several other arguments in favour of the truth of the sliding theory; from all which he inferred, that the movement was not a continuous but an interrupted process;—that when the melting of the sides of the mass detached it from its attachment to the sides of the valley, and it became undermined below, by the melting of its base, the force of gravity, unresisted by friction, was brought into play, and it made a sudden progressive movement (which might be only an inch or several feet), when it remained at rest, till the same causes produced a renewal of the same result. He shewed, that though many parts of these icy masses were nearly level, all the upper portions, and many of the lower, were lying over such inclined planes, that gravity could exert its full power in their propulsion; and as the whole icy mass was tolerably solid and continuous, the greater movement of one portion was communicated more or less throughout its whole length, and tended to urge forwards and downwards those parts which had less tendency to move onwards of themselves.

The author also endeavoured to account for the advance of one glacier, and the retirement of another alongside of it, by supposing that it was caused by the snows being drifted away from the one valley exposed to the blast, and from which the glacier, which was retiring, descended, and being deposited in deep wreaths in the other, which was probably more sheltered, and from which descended the glacier, which was making destructive advances. The increased accumulation of snow, by furnishing a supply greater than the waste, caused the one glacier to advance, whilst the other retired, in consequence of the waste at its lower extremity exceeding the supplies from above.

2. On Plague, in relation to the question of its Nature, whether or not a Contagious Disease. By John Davy, M.D, F.R.S.S. L. & E.

3. Analysis of Two New Minerals of the Zeolite Family

By Thomas Anderson, M D. Communicated by Dr Christison.*

4. Dr Christison exhibited specimens from the Government Superintendant of Tea Culture in Assam, illustrating the several ages at which the leaves of the Assam and China Tea-plants are used for making the different commercial varieties of black and green tea.

An examination of these specimens seemed to prove, that the leaves of the China tea-plant, cultivated at the same plantation with the tea-plant of Assam, are considerably less, and somewhat thicker, but otherwise so exactly similar, that the two plants may well be mere varieties of the same species,—an opinion now generally adopted by botanists in India. The specimens further illustrated the doctrine deduced from recent investigations in India, that the different kinds of green and black tea are made from the leaves of one species of plant, collected at different periods of their development. The specimens were collected in April 1841. The unexpanded shoots and very young leaves are marked as yielding Pekoe, a black tea, and Young Hyson, a green tea, by different modes of preparation. The fully-expanded, but still young leaves, are stated to produce Pouchong, Souchong, and Campoi, among the black teas, and Imperial, Gunpowder, and Hyson, among the green teas. Older and firmer leaves produce Congo, a black tea, and Twangkay and Hyson-skins, two of the green teas; and the oldest and coarsest of the leaves produce Bohea, the lowest in quality of the black teas.

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SCIENTIFIC INTELLIGENCE.

GEOLOGY AND GEOGRAPHY.

1. *M. Elie de Beaumont on the former low Temperature of European Winters.*—When speaking of winters sufficiently cold to admit of large ice-bergs floating in great numbers as far as latitude 50° (see Charpentier's Paper, at p. 59. of the present Number), M. E. de Beaumont says :—At first sight, this supposition appears contrary to the hypothesis so generally admitted, that the terrestrial globe was warmer during former geological periods than it is at present, and that it has been subsequently gradually cooled. This apparent opposition ceases, however, when we consider that the temperature of a given portion of the globe during a given time, depends not only on the general temperature of the globe, but also on the manner in which the Isothermal Lines were disposed, during that same period, under the influence of seas and of mountains whose configuration was quite different from the configuration of the seas and mountains of the present day. The globe, during the period which preceded ours, may as a whole have been a little warmer than it now is, and yet central Europe may have had a climate similar to that of Canada, where the phenomenon of the transport of blocks of rock by ice has been observed in latitude 48° or 50°. This supposition of colder winters in Europe, during the geological period preceding our epoch, would, moreover, be in harmony with many other observations. (*Comptes Rendus*, vol. xiv. p. 101.)

2. *Determination of the Amount of Depression of the Dead Sea below the level of the Mediterranean.*—In an article published in the 29th vol. of this Journal, p. 96, we detailed, at considerable length, the various conclusions regarding this depression deduced from the observations and experiments of Schubert, Moore and Beek, Bertou and Russegger. Since that time its amount has been estimated at 1200 feet from data obtained by the late Sir Daniel Wilkie. We are glad to find, by the following remarks contained in Mr Hamilton's address to the Geographical Society of London, that this interesting problem has now been completely and satisfactorily solved by Lieutenant Symonds of the Royal Engineers:—"This officer, during the last year, carried a line of levels across from Jaffa to the Dead Sea by two different routes; and the results, corresponding to within an insignificant fraction, give 1311.9 feet for the depression of the Dead Sea below the level of the Mediterranean, being a very few feet less than that given by M. Bertou. Lieutenant Symonds, by the same operations, found the level of the Lake of Tiberias to be only 328 feet below that of the Mediterranean, making an inclination of nearly 1000 feet between this lake and the Dead Sea, a distance of about 70 miles."

3. *On the Grooves and Polished Surfaces at the contact of Ancient Secondary Strata.*—Professor Rogers, at a late meeting of the Association of American Geologists, made some remarks respecting the grooved and polished surfaces at the contact of ancient secondary strata. He thinks he has seen unequivocal instances of these in Pennsylvania. Their production, at periods when the earth's temperature was manifestly incompatible with the existence of ice, would seem to demonstrate that angular detrital matter, urged by water, is able of itself to score and polish the surfaces of rocks.

Professor W. B. Rogers continued the illustration of this subject, by calling attention particularly to the evidences of ancient denudation and drifting action, so strikingly displayed along the place of junction of the Oriskany sandstone (Formation VII., of the Reports), and the subjacent limestones Formation VI.). In many districts the limestone has been irregularly denuded, and even to a great extent removed; and at the same time fragments of the limestone and fossils, water-worn and blended with coarse sand and gravel, have been accumulated to form the lower beds of the Oriskany rock. The rapid fluctuation in thickness of the upper limestones, as witnessed in Virginia, Pennsylvania, and Western New York (near Black Rock, for example), Professor R. ascribed rather to the irregular force of the denudation, than to irregularity of thickness in the original deposit. He dwelt upon the epoch of this limestone series, and the commencement of the overlying sandstone, as one of great interest in the history of the Appalachian rocks, marked as it is, throughout a great part of the Appalachian belt, by evidences of a sudden and great change in the physical conditions of the ancient sea, and by the proofs of attendant drifting and denuding action of extraordinary energy. He contended that the grooved and worn surfaces of the limestone which mark the abrading action of a drift at this ancient period, together with the same phenomena observed in the rocks of other portions of the Appalachian series, as described by Professor H. S. Rogers and Mr Hall, bear so striking a resemblance to those more recent effects, which have given rise of late to such deeply interesting speculations, that it would seem unphilosophical to refer the two to *different* mechanical causes. He therefore maintained, that as in the production of these ancient phenomena of diluvium or drift, it can hardly be supposed that ice, either floating or in the form of glaciers, could have performed any part, since the existence of ice in the ocean at that period is scarcely conceivable, we are under no necessity of resorting to the glacial, or even the glacio-aqueous theory, in explanation of the more modern phenomena of grooved and striated rocks.—*Silliman's American Journal of Science and Arts*, vol. xliii., No. 1, p. 181

4. *Geological Maps of Piedmont, &c.*—We are informed that Sismondi's Geological Map of Piedmont and Savoy will shortly appear, and that Pareto's Geological Map of the Duchy of Genoa and County of Nice is nearly finished.

5. *Humboldt's "Fragmens Asiatiques."*—We are glad to hear that Humboldt is actively engaged in the preparation of a second edition of his Fragmens Asiatiques.

6. *Heights of Localities in the Holy Land ascertained Barometrically by Russegger.*—Monastery of St Catharine on Sinai 5115 Parisian feet above the sea; summit of Dschebel Horeb, 7097; summit of Dschebel Catharine, 8168; Jericho, 717 below the sea; bathing place of the pilgrims in the Jordan, 1291 below the sea; Catholic Convent at Nazareth, 1161 above the sea; summit of Tabor, 1755; surface of the Lake of Tiberias, 625 below the sea; Dschebel Makmel, above Tripolis, the highest point of Lebanon, 8800 above the sea; the Cedars of Lebanon, above Eden, 6000; mountain pass between Beirout and Baalbeck, 5485; Bseddin coal-mines, 2906; Makla-ain-el-Bed coal-mines, 2873; Mar-hanna-el-Kennise coal-mines, 1803; Room at Beirout, 60; mountain pass from Beirout to Damascus, 4886; town

of Sebedäni, 4024; the Fall of Barada, at the Pass of el-Suk, 3346; town of Baalbeck, 3196; Damascus, 2304. The mountain elevations in Lebanon and Antilebanon are older than those in Southern Syria. The former belong to the chalk formation, but the latter to the tertiary deposits. This fact seems to correspond perfectly with the physical characters of the surface.—(*Poggendorff's Annalen*, 1841, No. 5.)

MINERALOGY AND CHEMISTRY.

7. *Dr Traill's Collection.*—We understand that Dr Traill wishes to dispose of his extensive and valuable Mineralogical and Geological Collection. The specimens of minerals amount to 3000, the rocks to 1500, and the organic remains to 500; in all about 5000. The whole are carefully named and catalogued, and arranged in handsome cabinets. The mineralogical department is rich in Fluors, Barytic minerals, Leads, Salts of Copper, Zeolites (particularly Apophyllites), Felspars, Scapolites, the scarcer Swedish and Norwegian minerals, the ores of Silver and Tellurium, Meteoric stones, &c.; and includes among the greater rareties, a superb crystal of Euclase (1 inch long, by ½ an inch broad, quite transparent, and finely acuminated); Gold in the matrix from Lead Hills; Stromnite or Barystrontianite (discovered by Dr Traill), &c. The geological series is illustrative more especially of Scotland, Spain, Brazil, Greenland, the Arctic Regions, &c.; and, among the fossil remains, there is a fine set of fishes from the Orkney Islands, named by Agassiz.

8. *Potash and Lime in Flint.*—It is known from Klaproth's analysis, that flint contains lime; but Berzelius has also found potash in the flint of the chalk of Limhamn, in Schonen. In 1000 parts of flint he detected 1.17 parts potash, and 1.13 parts lime, with traces of oxide of iron and alumina, and likewise a small quantity of a carbonaceous matter, which left no residue on being ignited, and which probably produces the colour in flint resembling the tint of brown rock crystal (*Rauchtopas.*) The analysis was undertaken with the view of ascertaining the cause of the decomposition of the surface of a flint knife, a change not unfrequently observed in flint exposed to the action of the atmosphere. The result obtained was, that the interior and undecomposed portion of this knife contained in the 1000 parts 1.34 potash; 5.74 lime; and 1.2 oxide of iron and alumina. The decomposed portion, on the other hand, which could easily be rubbed off in the state of powder, contained in the 1000 parts, 3.2 parts of potash, and 3.2 parts of lime; whence it would seem that the decomposition had its origin in a long continued action of a liquid containing potash, which gradually replaced the lime

by potash. The decomposition proceeded progressively, so that it had already evidently commenced in the still coherent portion of the flint, and had formed a white stripe round the mass, having a breadth of 0.3 to 0.4, decimal lines.—*(Berzelius' Jahres-Bericht*, xxi. *Jahrgang*, ii. *Heft*., p. 187.

9. *Amphodelite.*—Breithaupt has found that Amphodelite and Diploite (Latrobite, *Brooke*) not only resemble each other in colour and appearance, but also in the angles of their cleavage planes, and in the proportions of their constituent parts. The Diploite, however, contains, according to C. G. Gmelin's analysis, 6½ per cent. of potash, while that alkali is entirely awanting in the amphodelite, and is there replaced by a larger amount of lime and magnesia.—*(Berzelius' Jahres-Bericht, Jahrgang* xxi, p. 202.)

10. *Andesine.*—Abich has analysed a mineral to which he has given this name, and which is from the Andes. It was formerly termed Pseudoalbite, from its resembling greatly, in crystalline form, the twin crystals of Albite; but it presents a less distinct cleavage than that mineral, and its cleavage-surfaces are not so well defined. The Andesine is imbedded in a greyish-white mass, which is termed Andesite, and has a specific gravity of 3.5924; and it is mixed with hornblende and quartz, on which the crystals broken out leave a shining impression. The specific gravity of the Andesine is 3.7328, therefore greater than that of Albite. In thin splinters it melts before the blow-pipe, and in grains it fuses into a vesicular slag. The analysis with carbonate of Baryta gave—

		Oxygen.		
Silicic acid, ...	59.60	......	30.90	8
Alumina, ...	24.28	11.22	11.70	3
Oxide of iron, . .	1.58	0.48		
Lime, ...	5.77	1.61	3.79	1
Magnesia, ...	1.08	0.37		
Soda, ...	6.53	1.65		
Potash, ...	1.08	0.16		

It is therefore a Leucite, in which the greater proportion of the potash is replaced by Lime and Soda.—(*Berzelius' Jahres-Bericht, Jahrgang* xxi., *Heft* ii., p. 167.)

11. *Arquerite.*—In a report made to the French Academy of Sciences by MM. Berthier, Elie de Beaumont and Dufrenoy, on two memoirs by M. Domeyko, on the mineral products of the silver mines of Chili, there is an account given of a new native amalgam, which constitutes almost exclusively the riches of the silver mines of Arqueros, in the province of Coquimbo, in Chili. This amalgam *consists of six atoms* of silver, and one atom of mercury, a *composition presented* by no mineral previously analysed. Its com-

the production of the colour. The colouring matter in artificial ultramarines is partly of a fine blue, and partly of a fine green tint. The latter, by continued heating in an open vessel, passes into the former; which, Elsner says, takes place, because the blue colour requires the combination of a larger quantity of sulphur with the sodium. This is effected when a portion of the sulphur is converted into sulphuric acid, which removes soda from the sulphuret, by which means the latter becomes comparatively richer in sulphur. On this account the natural as well as the artificial ultramarine always contains sulphuric acid. (*Berzelius' Jahres-Bericht*, 1842.)

20. *Pennine.*—Fröbel has described, in Poggendorff's Annals, a mineral from the Matter or Nikolai valley, in the Pennine chain of the Alps, which has been analysed by Schweitzer, and has received the name of Pennine. It sometimes occurs in hexagonal tables, sometimes in streaked prisms; its colour is blackish green to leek green; it is translucent in thin plates; but, at right angles to the principal axis, it is brown or hyacinth red. Hardness between that of gypsum and calcareous spar. Its composition is—

			Oxygen.	
Silica,	33.82	33.07		17.07
Alumina,	9.32	9.69		4.35
Oxide of Iron,	11.30	11.36	2.52	15.31
Magnesia,	33.04	32.34	12.79	
Water,	11.50	12.58		10.22
	98.98	99.04		

It does not seem possible to deduce a proper formula from the result of the analysis. It is plain, however, that its composition approaches that of the chloritic minerals. (*Berzelius' Jahres-Bericht*, 21*st Jahrgang*, p. 177.

21. *Platina in the Auriferous Sand of the Rhine.*—M. J. Döbereiner has detected a small quantity of platina in the auriferous sand of the Rhine. The amount was only 0.4 of a grain in 2 ounces.

22. *Villarsite.*—M. Dufrenoy (*Comptes Rendus*, vol. xiv. p. 697) has given the designation to a new mineral, in honour of M. Villars, the author of a Natural History of Dauphiny. It was recently found by M. Bertrand de Lom in a vein of iron-ore at Traversella, in Piedmont, and is there associated with dolomite, mica, quartz, and dodecahedral crystals of magnetic iron-ore. The Villarsite has a yellowish-green colour, and a granular fracture; and in these respects bears a considerable resemblance to some of the Arendal apatites. The primitive form is a right rhombic prism of 119° 59′, and the crystals examined were rhombic octahedrons with truncated summits. The following is its composition—

		Oxygen.	
Silica, ...	39.60	20.57	4
Magnesia, ...	47.37	18.37	4
Protoxide of iron, . . .	3.59	0.69	
Protoxide of manganese, .	2.42	0.53	
Lime, ...	0.53	0.14	
Potash, ...	0.46	...	
Water, ...	5.80	5.14	1
	99.77		

Hence the formula is 4 Mg S + Aq, and the Villarsite is to be regarded as a monosilicate of magnesia. Except that it contains water, this newly discovered substance has the same composition as crysolite; but, while the proportion of water is too large to admit of its presence being regarded as accidental, the external, crystallographic, and chemical characters are opposed to its being united with that species. The Villarsite furnishes a new example of a mineral associated with Plutonic crystalline products containing water of crystallisation. M. Dufrenoy remarks, that we are already in possession of analyses which prove the presence of water in rocks evidently volcanic, and hence concludes, that it is not necessary to have recourse to the theory of infiltrations for the explanation of the occurrence of zeolites in basalts, trachytes, and even in traps.

23. *Xenolite.*—This new mineral is so named from its not belonging to the locality where it is found. It occurs along with Wörthite, near Peterhoff, in boulders, which are probably derived from Finland. It is crystallized in prisms, united together in very delicate fibrous masses. On being separated, the fibres are found to be three-sided prisms, in which two of the sides form an angle of 45° 38′, and the third seems to be at right angles to one of the others. There is a terminal plane. Hardness = that of quartz. Sp. gr. = 3.58. It is colourless, but occasionally presents greyish or yellowish portions. Translucent. Fracture uneven, granular. Lustre vitreous, and, on the more distinct cleavages, pearly. Gives no water before the blowpipe. Infusible in fragments and in powder. Fusible with difficulty, along with borax and phosphate of soda. According to an analysis by M. Komonen, this mineral consists of silica 47.44, and alumina (with a little oxide of iron) 52.54 = 99.98. (*Poggendorff's Annal.* 1842, No. 8, from paper by Nordenskiöld in the *Act. Soc. Scient. Fennicæ*, vol. i. p. 372.)

24. *Sulphuric and Molybdic Acids.*—Dr Thomas Anderson of Leith has lately made some experiments on the relations of these two acids. The molybdic acid dissolves in the sulphuric, but the combination cannot be made to crystallize by evaporation. However, on decomposing molybdate of baryta with an excess of sulphuric

acid, and evaporating the solution over sulphuric acid, a crystallized compound is obtained, which, according to the analysis of Anderson, consists of sulphuric acid 57.3, molybdic acid 32.8, water and loss 9.9. Two isomeric modifications seem to be indicated.—(*Berzelius' Jahres-Bericht*, 1842.)

25. *Calcareous Rocks pierced by Helices.*—M. Constant Prévost exhibited to the Société Philomatique de Paris, numerous specimens of a very compact grey limestone, which appeared to him to have been deeply perforated by Helices. He collected these specimens himself, in 1831, on *Monte Pelegrino*, near Palermo, at an elevation of about 200 metres above the level of the sea. He at first supposed that the perforations were the work of marine lithophagous mollusca, and that they indicated one of the levels of the sea at a remote period; but the irregular and sinuated form of the cavities,—their depth (extending to 12 and 15 centimetres),—their dimensions (being from 4 or 5 millimetres to 4 centimetres in breadth),— and above all, the presence of a Helix of different ages, belonging to the same species, and each individual lodged in a cavity exactly proportioned to the dimensions of the shell,—led him to the belief that the Helices had themselves scooped out their abode. The difficulty, however, of understanding how they could accomplish this, made him hesitate in announcing publicly the fact he had observed, until new facts, and more direct and positive observations, had confirmed his opinion. He carefully collected fragments of the perforated rock, and the Helices which inhabited it.

In 1839, when the Geological Society of France met at Boulogne-sur-mer, M. Constant Prévost, along with Messrs Buckland and Greenough, who attended the meeting, discovered perforations precisely analogous to those of Palermo in an equally hard limestone in the neighbourhood of Boulogne (the mountain limestone), and Dr Buckland, on breaking the perforated rock, found many Helices at the bottom of the cavities.

This new instance, although strengthening the presumption arising from the fact observed at Palermo, did not yet definitely settle the question—Had the Helices pierced the stone, or had they merely taken advantage of the old perforations of marine lithophagous molluscs, and converted them into a residence? At the meeting of the British Association at Plymouth, in 1841, Dr Buckland remarked, in reference to a Memoir by Mr Walker, on the destructive action of Pholades, that all the perforations observed in calcareous rocks are not necessarily the work of marine molluscs, and he mentioned Helices as likewise perforating stones, supporting this assertion by the observation made at Boulogne in 1839, and even adding that Mr

Greenough had positively ascertained the action of the *Helix aspersa* on limestone.

To the facts above narrated, and the authorities just cited, M. Constant Prévost adds a circumstance which appears to him to confirm his first idea, and to render it unquestionable that the Helices have themselves scooped out the long canals at the bottom of which we find them. He pointed out the fact, in one of the specimens presented to the Society, that the bottom of one of the largest cavities presented an exact counterpart to the form of the Helix which lodged in it: a small projection corresponds exactly to the depression at the origin of the column, and, by taking an impression of the cavity in plaster, he obtained a relief which in no respect differed from that of the base of the shell.

The Helix found at Boulogne-sur-mer was the common *H. aspersa*. That observed at Monte Pelegrino seemed to be a very remarkable variety of that species, at least it is so regarded by Rosmaesler, who has figured it under that name in his Iconographia of Land and Fresh-water Shells, pl. xxii. It is the Helix described and figured as distinct, under the name of *Helix Mazzuli* by Zau and Phillipi, and under that of *H. Retirugis* by Menke.

The same Helix, now found alive in the vicinity of Palermo, is met with in a fossil state in the marine tertiary deposits which surround the base of Monte Pelegrino. M. Constant Prévost further remarked, that it is by maceration, or by chemical action, and not by a mechanical action, that the Helix corrodes the stone. In fact, the compact limestone of Monte Pelegrino, which is a little argillaceous and bituminous, is traversed in every direction by numerous veins of crystalline limestone; these more resisting parts are seen projecting like a kind of net-work on the interior walls of the cavities, which could not have taken place if the calcareous matter had been removed by friction.

M. Constant Prévost terminates his communication by shewing how important it is that geologists should not confound the perforations which may have been produced in rocks by marine molluscs with those of Helices, since the former, observed at the present time on very elevated parts of continents, indicate ancient levels of the sea, or the relative elevations of the ground, whereas the perforations of the Helex indicate nothing of that nature.—*From L'Institut.*, April 1842, p. 132.

26. *On the residuum of the Combustion of the Diamond, by M. Petzholdt.*—By repeating the experiments of Messrs Dumas and Stass, in order to determine the atomic weight of carbon by the com-

bustion of the diamond, Messrs Erdmann and Marchand have obtained, like these chemists, a residuum of very small volume, scarcely perceptible in the case of small diamonds, and which consisted of a reddish substance, the parts of which sometimes presented a brilliant surface, and seemed as if they had been already formed and enclosed in the fissures of the burnt mineral. M. Petzholdt found that this residuum (which was not more than 0.0072 gram. in a diamond of 5.6344), consisted principally of a great number of small plates or scales, among which were found mingled, but very rarely, softer and more rounded parts. Under the microscope these bodies appeared some of them black and not transparent, others likewise black, but passing into brown, and a little transparent; others also were transparent, light brown, passing into yellow, and, finally, some were yellow or white. With regard to their internal structure, as far at least as it was disclosed by the microscope, it appeared to differ in an equal degree, particularly in such as were transparent and semi-transparent; generally it appeared granular in those that were transparent and white, radiated or plicate in the yellow. Sometimes black masses, similar to grains, might be observed here and there in the substance of the transparent splinters, as well as in the leaflets, which gave these portions a brownish aspect when they were looked at with the naked eye. The most interesting circumstance of all is, that in a great number of these bodies, we distinctly perceive a delicate net-work, black or deep brown, with hexagonal meshes, many of which often run into each other, and bear an absolute resemblance to those which the researches of the microscope discover in the parenchyma of plants. Sometimes this net-work appears to dissolve, or rather to have been affected in such a way that its contours appear to become confounded and disappear, while in the other parts of the same body it was perfectly entire.

These observations give rise to the conjecture, that this network, and the black substances which accompany it, are nothing more than the debris of vegetable carbon, the combustion of which could not take place simultaneously with that of the diamond, because they were surrounded by bodies incapable of burning.

The analysis of this residuum by means of the blowpipe for sale, shews that it consists of silica, with traces of iron.

On examining the diamonds of commerce at Dresden, and those of the mineralogical collection at the Royal Museum, M. Petzhold has again found among many of them the same plates or scales, and, in the middle of one of them, a small brown, *transparent*, triangular leaflet, in which he remarked one of these reticulations in question, although already in a state of dissolution. This seems to confirm the opinion of Messrs Erdmann and Marchand, that these bodies are all formed in the fissures of the diamond in which they are enclosed, and it tends to support the notions which M. Liebig has expressed in his Organic Chemistry, respecting the constitution of the diamond.—*From L'Institut.*, 21st July 1842, p. 260.

MISCELLANEOUS.

27. *Indian Isinglass.*—Isinglass, as is well known, is manufactured from the swimming-bladders or *sounds* of certain fish. Of these the large sturgeon, caught in several rivers of Russia, furnishes the best, or is the best prepared; selling by wholesale at 10s. to 12s. the pound, whilst the Brazilian or North American only fetches from 2s. 6d. to 3s. 6d., and there are inferior qualities realizing no more than 9d. The value of this seemingly trifling article to Russia may be inferred from the annual imports into England, which vary from 1800 to 2000 hundredweight.

After an occupation of Calcutta of more than a century, and a territorial possession of Bengal of eighty years, an individual, writing anonymously in a periodical, acquainted the Indian public with the novel facts, not merely that the waters of India produced in plenty fishes that would furnish isinglass, but that a trade in this commodity had long been carried on (it turns out from time immemorial) between the Indian fishermen and the Chinese, who, not satisfied with the products of the Ganges, ransacked the whole of the archipelago for parts of fish yielding isinglass, or a gelatinous substance very much akin to it. They have extended their researches even to Bombay; whence upwards of 5000 hundredweight of "shark fins and fish maws" were exported to China in 1837–38; fish maws, though known by name, being quite unknown in their nature till Dr Royle, after great difficulty, obtained specimens through the house of Forbes and Co. "On examination, these proved to be composed of a sack-like membrane, which had been split open, of a light colour, and semi-transparent, resembling the ordinary qualities of isinglass in appearance." It is also said that the Chinese, after exporting the roughly-cured Ganges isinglass, refine some of it, and reimport it at a large profit.

Attention has also been paid to the isinglass itself, specimens of which have been forwarded to Europe, some prepared under the inspection of Mr M'Cleland, of the Bengal medical service. The less scientifically-prepared samples were valued at 1s. 8d. and 4s. *per pound*; that prepared under the inspection of Mr *M'Cleland, of the Bengal* medical service, produced 1s. 7d.; the mere cost of which, in India, including the purchase and preparation, was only 1s. 1d. per pound; but subsequent expenses, and duties of various kinds, rendered the whole cost threefold the amount realized by the sale. Subjected to scientific analysis, the Indian isinglass differs but little from the Russian. It is of so much less market value, partly because it is new and the supply uncertain; partly from the form in which it has been brought to England, which is favourable to adulteration; but chiefly from the want of care in the preparation, an unpleasant fishy smell remaining, which renders it impossible to bring it into use here for culinary purposes. Some importations, however, have taken place, nor is the article now unknown to the London brokers; so that there is every prospect of a new and profitable source of commerce being opened to India, if care and capital be applied to the preparation of the isinglass.

28. *Ancient Fable of Colossal Ants producing Gold.*[*]—One passage will satisfactorily explain the extravagant fable related by the Greeks, and repeated by travellers in the middle ages, of ants as big as foxes, who produce gold. The passage states, that the tribes of various names who dwell between the Meru and Mandara Mountains, brought lumps of gold, of the sort called paippilika, or ant gold,—so named, because it was dug out by the common large ant or pipilika. It was, in fact, believed that the native gold found on the surface of some of the auriferous deserts of northern India had been laid bare by the action of these insects;—an idea by no means irrational, although erroneous, but which grew up, in its progress westward, into a monstrous absurdity. The native country of these tribes is that described by the Greeks, the mountains between Hindoostan and Thibet; and the names given are those of barbarous races still found in those localities.

29. *On the Transformations which have been produced in Turf by the Essence of Turpentine, or by a composition isomeric with it.* By M. Forchhammer.—Extensive researches have demonstrated that Denmark was formerly covered with a forest of firs, and that this vegetation had already disappeared at a period so remote, that there remains no historical or traditional trace of it. The stems and roots of magnificent firs are now found in the greater part of the peat-bogs of the country; and M. Steenstrup has recently discovered in these some crystals, which have such a resemblance

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[*] From a paper read to the Royal Asiatic Society, by Professor Wilson, "On a portion of the Mahabharata," &c.

to the scheererite of Uznach, in Switzerland, that they were at first taken for that mineral substance. M. Forchhammer, who has studied these crystals, has found that they are composed of two substances, to one of which he gives the name of *Tecoretine*, on account of the facility with which it enters into a state of fusion; to the other, that of *Phylloretine*, because it crystallizes in fine leaflets. These two substances may be separated, by dissolving the crystals in boiling alcohol.—*From L'Institut.*, June 16, 1842, p. 217.

30. *On the Preservation of Flowers.*—To preserve flowers fresh. It is now, alas! a long eighteen years ago since we first saw, in the drawing-room of a gentleman now no more, in the hot, dry weather of the dog-days, flowers preserved day after day in all their freshness by the following simple contrivance:—A flat dish of porcelain had water poured into it; in the water a vase of flowers was set; over the whole a bell-glass was placed, with its rim in the water. This was a "Ward's case" in principle, although different in its construction. The air that surrounded the flowers, being confined beneath the bell-glass, was constantly moist with the water that rose into it in the form of vapour. As fast as the water was condensed, it ran down the sides of the bell-glass into the dish; and if means had been taken to enclose the water on the outside of the bell glass, so as to prevent its evaporating into the air of the sitting-room, the atmosphere around the flowers would have remained continually damp. What is the explanation of this? Do the flowers feed on the viewless vapour that surrounds them? Perhaps they do; but the great cause of their preserving their freshness, is to be sought in another fact. When flowers are brought into a sitting-room they fade, because of the dryness of the air. The air of a sitting-room is usually something drier than that of the garden, and always much more so than that of a good green-house or stove. Flowers, when gathered, are cut off from the supply of moisture collected for them by their roots, and their mutilated stems are far from having so great a power of sucking up fluids as the roots have. If, then, with diminished powers of feeding, they are exposed to augmented perspiration, as is the case in a dry sitting-room, it is evident that the balance of gain on the one hand by the roots, and of loss on the other hand by their whole surface, cannot be maintained. The result can only be their destruction. Now, to place them in a damp atmosphere, is to restore this balance; because, if their power of sucking by their wounded ends is diminished, so is their power of perspiring; for a damp atmos-

phere will rob them of no water. Hence they maintain their freshness. The only difference between plants in a "Ward's case," and flowers in the little apparatus just described, is this—that the former is intended for plants to grow in for a considerable space of time, while the latter is merely for their preservation for a few days; and that the air which surrounds the flowers is always charged with the same quantity of vapour, but will vary with the circumstances, and at the will of him who has the management of it. We recommend those who love to see plenty of fresh flowers in their sitting-rooms in dry weather, to procure it. The experiment can be tried by inserting a tumbler over a rosebud in a saucer of water.—*Gardeners' Chronicle.*

NEW PUBLICATIONS.

We have received among others the following works, which we recommend to the attention of our readers:—

1. W. E. Redfield on Whirlwind Storms; with replies to the Objections and Strictures of Dr Hare. New York. 1842.

2. An Introduction to Entomology, or Elements of the Natural History of Insects; by Messrs Kirby and Spence. Two volumes 8vo. Longman, Brown, Green, and Longmans, London. 1843. *The sixth edition of these admirable volumes.*

3. Descriptive and Historical account of Hydraulic and other machines for raising water, ancient and modern; including the progressive development of the Steam Engine; by Thomas Ewbank. Illustrated by nearly three hundred Engravings. One volume 8vo, pp. 582. Tilt and Bogue, Fleet Street, London. 1842. The *English edition of a valuable, very interesting, and amusing work.*

4. Nomenclator Zoologicus, continens Nomina Systematica Genera Animalium, Tam viventium quam Fossilium; auctore L. Agassiz. Fasciculus II. continens Aves. Solodur, 1842. *This work, when finished, will become indispensable to every naturalist.*

5. Sketch of the Geology of Moray; by Patrick Duff, Esq. 8vo. With Plates. Forsyth and Young, Elgin. *A lucid geological account of a small but interesting district.*

6. On the Voltaic Circuit; by Alfred Smee, F.R.S.

7. Popular Conchology, or the Shell Cabinet arranged, being an Introduction to the modern System of Conchology; by Agnes Catlow. Illustrated by figures of all the genera. Small 8vo., pp. 300. Longman, Brown, Green and Longmans, London. *A pleasant, useful, and well illustrated volume.*

8. The employment of the Microscope in Medical Studies; by John Hughes Bennet, M.D., Lecturer on Clinical Medicine, &c. Maclachlan

and Stewart, Edinburgh. *An interesting discourse on a very popular subject.*

9. Memoire sur les Kaolins ou Argiles a Porcelaine; par MM. Alexandre Brongniart et Malaguti. 4to. Paris, 1841. *The most philosophical essay on the Porcelain Earth we have met with.*

10. Rede zum Andenken an Dr Ignaz Döllinger; von Dr Fr. v. Walther. München. 4to. 1841. *An excellent biography of a distinguished physiologist.*

11. On the Fossils of the Mountain Limestone in Ireland, as compared with those of Great Britain; by R. Griffith, F.R.S.E., &c. 4to. *A valuable geological document.*

12. Recherches sur certaines circonstances qui influent sur la Temperature du point d'ebuillition des liquides; par W. F. Marcet. 4to. 1842.

13. Elements of Electro-Metallurgy; by Alfred Smee, F.R.S. Parts 4, 5, 6, 7. Palmer, London. *A work now nearly completed, the best on Electro-Metallurgy in our language.*

14. Ninth Annual Report of the Royal Cornwall Polytechnic Society. 1841. J. Trathan, Falmouth. *The record of the ninth Session of a very useful association.*

15. What to Teach, and how to Teach, &c.; by H. Mayhew. 8vo. William Smith, London.

16. American Repertory of Arts, Sciences, and Manufactures. 1841. New York.

17. Proceedings of the American Academy of Sciences of Philadelphia, 1842.

18. Report of a Committee appointed by the British Association "to consider the rules by which the Nomenclature of Zoology may be established on a uniform and permanent basis." 1842.

19. Experimental Inquiry into the advantages attending the use of Cylindrical Wheels on Railways; by W. J. Macquorn Rankine, Esq., Civil Engineer. R. Grant and Sons, Edinburgh. *The first publication of a young and promising engineer.*

20. Memoir of William Maclure, Esq., late President of the Academy of Natural Sciences of Philadelphia; by S. G. Morton, M.D. Philadelphia, 1841. *The biography of an excellent man and active geologist.*

21. Boston Journal of Natural History. Boston.

22. Professor Silliman's Address before the Association of American Geologists and Naturalists. Held in Boston, April 25-30, 1842. *The best view of the present state of geology in America.*

23. Zoology of the voyage of H.M.S. Beagle. Edited by Charles Darwin, Esq., F.R.S. Part V. Reptiles by Thomas Bell, Esq., F.R.S. No. 1.

24. Illustrations of the Zoology of Southern Africa; by Andrew Smith, M.D., No. 16.

25. Journal of the Asiatic Society of Bengal.

26. Report of Mr Owen's Monograph on the Apteryx Australis.

27. The Maryland Medical Journal.

List of Patents granted for Scotland from 26th September t *22d December* 1842.

1. To Charles William Firchild, of Wesley Park, in the parish o Northfield, in the county of Worcester, farmer, "an improved propellin apparatus for marine and other purposes."—26th September 1842.

2. To Edwin Ward Trent of Old Ford Bow, in the county of Middlesex, rope-maker, "an improved mode of preparing oakum and other fibrous substances for caulking ships and other vessels."—29th September 1842.

3. To Peter Kagenbusch, of Wetter on Rhur, in Westphalia, in the kingdom of Prussia, dyer, now residing in the parish of Lyth, in the county of York, in England, "certain improvements in the treatment of the alum rock or schist, and in the manufacture and application of the products derived therefrom."—29th September 1842.

4. To Henry Bewley, of Dublin, in the county of the city of Dublin, licentiate apothecary and chemist, "an improved chalybeate water."—4th October 1842.

5. To Alfred Jeffery, of Lloyd's Street, Pentonville, in the county of Middlesex, gentleman, "a new method of preparing masts, spars, and other wood for ship-building and other purposes."—18th October 1842.

6. To Claude Edward Deutsche, of Fricour's Hotel, St Martin's Lane, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in combining materials to be used for cementing purposes, and for the preventing the passage of fluids, and also for forming articles from such composition of materials."—18th October 1842.

7. To John Ridsdale of Leeds, in the county of York, "improvements in preparing fibrous materials for weaving, and in sizing warps."—20th October 1842.

8 To Samuel Carson, of York Street, Covent Garden, in the county of Middlesex, gentleman, "improvements in purifying and preserving animal substances."—20th October 1842.

9. To Henry Brown, of Selkirk, manufacturer, and Thomas Walker, of the same place, manufacturer, "improvements on woollen-carding engines." —20th October 1842.

10. To Alphonse de Troisbrioux, of Great Russel Street, Bloomsbury, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in lithographic and other printing presses."—20th October 1842.

11. To John Varley, of Colne, in the county of Lancaster, engineer, and Edmonson Varley of the same place, cotton-manufacturer, "certain improvements in steam-engines."—26th October 1842.

12. To James Hyde of Duckenfield, Cheshire, mechanic, and John Hyde of the same place, cotton-spinner and manufacturer, "a certain improvement or improvements in the machinery used for preparing cotton, wool, silk, flax, and similar fibrous material for spinning cotton."—3d November 1842.

13. To John Clay, of Cottingham, in the county of York, gentleman, and Frederick Rosenborg of Sculcoates, in the county of York, gentleman, "improvements in arranging and setting up types for printing."—3d November 1842.

14. To James Pilbrow, of Tottenham Green, in the county of Middlesex, engineer, "certain improvements in the application of steam, air, and other vapours and gaseous agents to the production of motive power, and in the machinery by which the same is effected."—7th November 1842.

15. To Francis Roubiliac Conder, of Highgate, in the county of Middlesex, civil-engineer, being a communication from abroad, "improvements in the cutting and shaping of wood, and in the machinery for that purpose."—9th November 1842.

16. To John Mitchell, of Birmingham, in the county of Warwick, steel-pen manufacturer, "a certain improvement in the manufacture of metallic pens, and a certain improvement in the manufacture of penholders."—11th November 1842.

17. To Henry Clarke, of Drogheda, in the county of Louth, in the kingdom of Ireland, linen merchant, "improvements in machinery for lapping and folding all descriptions of fabrics, whether woven by hand or power."—17th November 1842.

18. To John Spinks, the younger of John Street, Bedford Row, in the county of Middlesex, gentleman, "an improved apparatus for giving elasticity to certain parts of railways, and other carriages requiring the same," being a communication from abroad.—21st November 1842.

19. To Thomas Wrigley, of Bridge Hall Mills, Bury, Lancaster, paper manufacturer, "certain improvements in machinery for manufacturing paper."—28th November 1842.

20. To William Coley Jones of Vauxhall Walk, in the parish of Lambeth, in the county of Surrey, chemist, "improvements in treating or operating upon a certain unctuous substance, in order to obtain products therefrom, for the manufacture of candles and other purposes."—7th December 1842.

21. To Charles Maurice Elizee Sautter, of Austin Friars, in the city of London, gentleman, being a communication from abroad, "improvements in the manufacture of sulphuric acid."—7th December 1842.

22. To Don Pedro Pouchant, of Glasgow, civil-engineer, "a certain improvement or improvements in the construction of machinery for manufacturing sugar."—7th December 1842.

23. To Charles Heard Wild of Birmingham, in the county of Warwick, engineer, "an improved switch for railway purposes."—7th December 1842.

24. To John Browne, of Charlotte Street, Portland Place, in the county of Middlesex, Esquire, "improvements in the manufacture of mud-boots and overalls."—7th December 1842.

25. To William Coley Jones of Vauxhall Terrace, in the county of

Surrey, practical chemist, and **George Fergusson Wilson** of Vauxhall in the same county, gentleman, "improvements in operating upon certain organic bodies or substances, in order to obtain products or materials therefrom, for the manufacture of candles and other purposes."—7th December 1842.

26. To **William Losh**, of Newcastle-on-Tyne, Esquire, "improvements in the construction of wheels for carriages and locomotive engines intended to be employed on railways."—9th December 1842.

27. To **Thomas Cardwell** of Bombay, in the East Indies, merchant, "improvements in the construction of presses for compressing cotton and other articles."—9th December 1842.

28. To **Charles Augustus Preller**, of East Cheap, in the City of London, merchant, being a communication from abroad, "improvements in machinery for preparing, combing, and drawing wool and goat's hair."—9th December 1842.

29. To **Thomas Seville**, of Royton, in the county of Lancaster, cotton-spinner, "certain improvements in machinery used in the preparing and spinning of cotton, flax, and other fibrous substances."—9th December 1842.

30. To **William Young** of Queen Street, in the city of London, lamp-maker, "improvements in lamps and candlesticks."—12th December 1842.

31. To **George Edmund Donisthorpe**, of Bradford, in the county of York, top-manufacturer, "improvements in combing and drawing wool and certain descriptions of hair."—12th December 1842.

32. To **John Bishop** of Poland Street, in the county of Middlesex, jeweller, "improvements in apparatus used for retarding carriages on railways, parts of which are applicable for portioning power, and improvements in steam-cocks or plugs."—12th December 1842.

33. To **Isham Baggs**, of Wharton Street, in the county of Middlesex, chemist, "improvements in the production of light."—13th December 1842.

34. To **Gabriel Hippolite Moreau**, of Leicester Square, in the county of Middlesex, gentleman, "certain improvements in steam-generators."—13th December 1842.

35. To **John George Bodmer** of Manchester, in the county of Lancaster, engineer, "certain improvements in the manufacture of metallic hoops and tyres for wheels, and in the method of fixing the same for use, and also improvements in the machinery or apparatus to be employed therein."—19th December 1842.

36. To **William Lomas** of Manchester, in the county of Lancaster, worsted-spinner, and **Isaac Shimwell**, of the same place, worsted-spinner, "certain improvements in the manufacture of fringes, cords, and other similar small wares; and also in the machinery or apparatus for producing the same."—21st December 1842.

37. To **Moses Poole** of Lincoln's Inn, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in dressing mill-stones."—22d December 1842.

38. To **William Palmer** of Sutton Street, Clerkenwell, in the county of Middlesex, manufacturer, "improvements in the manufacture of candles."—22d December 1842.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Sketch of the Writings and Philosophical Character of Augustin Pyramus Decandolle, Professor of Natural History at the Academy of Geneva, &c. &c.[*] By Charles Daubeny, M.D., F.R.S., &c., Professor of Chemistry and of Botany in the University of Oxford. Communicated to this Journal by the Author.

The name of Decandolle is, I conceive, familiar to the ears of most persons of education, as that of an individual eminent in the ranks of modern naturalists—holding a place amongst botanists of the age which has just gone by, similar to that which Linnæus and Tournefort might have filled at an antecedent epoch, or which Brown and Hooker occupy in the present.

But I question, nevertheless, whether those I now address are in general acquainted with the peculiar grounds upon which his scientific reputation is based, and whether they may not regard him simply as one of those individuals who signalized themselves in their day, either by the discovery of new plants, or by their extensive acquaintance with those which the researches of others had already brought to light.

Were such the case, I certainly should not have chosen for the subject of a communication to the Ashmolean Society a topic like the present; for although prompted to the task now entered upon by a sense of the obligations I owe to this great botanist, not only in common with all who have studied his works, but also more particularly for many acts of personal kindness, and much information liberally afforded me during my former residence at Geneva; yet I should despair of being able to interest you in my delineation of his scientific character, if accuracy of observation, and a retentive memory, applied to the subject-matter of botany, had constituted the only traits by which he stood remarkable amongst his fellows.

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[*] Read before the Ashmolean Society of Oxford, February 13, 1843.

But I flatter myself, that a sketch of his several contributions to science, and of the qualities of mind displayed in his mode of handling the subjects they embrace, will possess some interest, not only as it may lead to a higher estimate of the branch of natural history to which they relate, but also because it will enable you to trace the steps by which a great mind was enabled to ascend to many important general principles, not by mere happy guesses at truth, but by a gradual and laborious accumulation of facts—a power of assimilating, as it were, and combining into an harmonious whole, the discoveries of other men, together with a singular sagacity in deducing conclusions from the data he had thus collected.

Augustin Pyramus Decandolle was born at Geneva in the year 1778, within a month, it has been remarked, of the death of Linnæus.* He was distinguished from his infancy by a most retentive memory,† and by a fondness and aptitude for study; but it is remarkable, that his earliest tastes were exclusively literary, and that he had acquired in his boyhood a great facility in composing verses, which, indeed, he retained ever afterwards, though I am not aware of any poetry having been published under his name. To these literary occupations of his youth, antecedent to his devotion to natural history, I should be disposed to attribute, the purity of his language, the remarkable clearness and sustained energy of his style, and the absence at once of those affectations, and those involved periods, which too often disgust or embarrass us in the writings of other men of science.

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* Also, as Flourens states, two months after the death of Haller, and three months after that of Bernard de Jussieu.

† He has been known to repeat every word of a copy of verses after hearing them once recited.

Those who have perused the works of the late Sir John Leslie, or of the still more celebrated John Hunter, not to allude to men of less name and distinction, will be sensible, by the aid of contrast, how much the reception of scientific truths is promoted by the power which Monsieur Decandolle had acquired, from an early familiarity with the purest models of style, no less perhaps than from his own natural clearness of conception, of presenting before us, without study or premeditation, that copious flow of ideas with which his mind was fraught on all subjects connected with his favourite science, in language so perfectly precise, and in an order so completely methodical.

At length, after he had in some measure satiated himself with the sweets of elegant literature, a love for botany appears to have been awakened in his mind by an attendance on the lectures of Professor Vaucher* of Geneva, who lived long enough to have the satisfaction, at a later period, of seeing his former pupil in undisputed possession of the foremost rank amongst European naturalists.

At the age of 18, in the year 1796, he went to Paris,† where a taste for physical science, which had been suspended for a while by the atrocities and by the vandalism of the Revolution, began to revive.

Here he attended the lectures of Vauquelin, Cuvier, Fourcroy, and others, and contracted a friendship with Desfontaines and Lamarck.

The former had, in 1787, established that important generalization, with respect to the essential differences pervading plants with one cotyledon and with more, which I have ventured on a former occasion* to characterize "as the keystone of the natural system, and as holding the same rank in botany, which the discovery of the circulation of the blood, or the distinction between vertebrated and invertebrated animals, claims in zoology."

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* Flourens, in his Eloge of Decandolle, which has reached me since the present memoir was drawn up, attributes the awakening of a taste for botany in the mind of Decandolle to another circumstance, namely, to his taking refuge, when a boy, with his mother and brother, whilst the French were besieging Geneva in 1792, in a village situated at the foot of the Jura, where he amused himself in collecting wild plants. The statement given in the text was taken from the sketch of Decandolle's life given in the Federal newspaper by a distinguished fellow-citizen of Geneva; and it seems probable that both causes may have contributed to give him this early bias.

† At the suggestion of Dolomieu, according to Flourens.

The latter had already promulgated those singular speculations respecting the origin of inorganic matters, intended by him to supersede the new chemistry, which Lavoisier had so recently founded on the basis of experiment.

In these it had been assumed, that life was the original cause of all combinations, the antagonist to those natural forces, which tend to resolve the elements of matter into their simplest forms, and which bring about death in organic, and dissolution in inorganic substances.

But although such immense effects were attributed to the operation of life, Lamarck had not yet explained to the public how he considered this principle to operate; and it was only in 1802 that we find him, in his "researches on the organization of living bodies," attributing to that blind impulse, or creative energy, which he denominates life, the power of building up, by an indefinite succession of efforts, the complicated organization of an animal or a plant.

It is probable, however, that these theories were floating in his mind at the time when Decandolle's intimacy with him commenced, and must have formed the subjects of frequent discussion, thus serving to render the latter familiar with those facts respecting abortive and rudimentary organs, on which the French Naturalist had raised this fanciful and airy superstructure.

That a connexion with such persons as I have mentioned, should impart a bias to the genius and pursuits of a young man just entering into life, was unavoidable; but what may be remarked as the peculiar merit of Monsieur Decandolle was, that whilst we may trace in his writings the impress of those principles of science, which might be gleaned from the writings of both the above mentioned philosophers, we shall find them in his writings expanded by more extensive information, and corrected by a sounder and severer judgment.

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* See my Inaugural Lecture on the Study of Botany, Oxford, 1834.

Thus he adopted the distinction between monocotyledonous and dicotyledonous plants from Desfontaines, and the doctrine of abortive and rudimentary parts from Lamarck; but the former truth was exhibited by him, not in the form of the bare announcement of a great principle, but as the very foundation on which all his systems, both in physiological and descriptive botany, were based; whilst the latter never became in his hands the pretext for any such chimerical and dangerous speculations, as were associated with them in the mind of their originator.

The earliest publications, however, of a botanical kind in which Decandolle's name figures, were calculated to display his power of accurately discriminating species, rather than the philosophical character of his genius.

In 1802 he published the first part of the description of Succulent Plants, drawings of which were supplied by the celebrated Redouté.

He likewise, about the same time, drew up a description of the Liliaceæ for the same author, and published a folio volume on the Astragalus and its allied genera.

In 1804 he obtained his degree of Doctor of Physic, and delivered on that occasion a thesis on the Medical Properties of Plants, which served as the basis of a work on that subject, brought out by him in 1816, shewing that he was already alive to the connexion that subsists between the natural structure of plants and their medicinal virtues.

In the same year he delivered, at the College of France, his first course of lectures on the Principles of Botanical Arrangement, of which he has given a sketch in the introduction to the Flore Française published the following year.

Although this essay may not have attracted all the attention it deserved, in consequence of making part of a Flora, a kind of work in which persons in general do not look for principles of physiology; yet it contributed in no slight degree to the establishment of correct principles of classification, and served as the basis of the Treatise which he published on this *branch of the subject* some years afterwards.

We thus see that the germs of two of his most important publications existed in the mind of M. Decandolle at an early period of his life, for in 1804, when he delivered his inaugural dissertation, and gave his first course on Botany, he was only 26 years of age.

The basis also of two other great undertakings was laid at a period not much later, for in 1805 commenced, as I have already stated, the publication of the third edition of the Flore Française, under the joint auspices of Lamarck and Decandolle; and in 1806, we owe to the subject of this sketch a Botanical Chart, in which France is divided into six regions, distinguished by the character of their respective vegetations, to which are appended some remarks on the geographical distribution of plants, serving as a prelude to that more detailed exposition of the subject, which we shall find to have been given, in the year 1820, in the Dictionnaire des Sciences Naturelles.

The former editions of the Flore Française, as Cuvier observes,* had no pretensions to be considered as a complete history of the species of plants indigenous to France,—their aim was rather that of exemplifying, by means of the plants which former botanists had enumerated, the peculiar artificial method of determining the name of a species, which Lamarck had proposed as a substitute for the then popular one of Linnæus.

This system consists in setting out with the most general forms, dividing and subdividing always by two, and only allowing the choice between two opposite characters, so as to conduct the reader, step by step, almost infallibly to the determination of the plant of which he desires to discover the name.

The services, therefore, which Decandolle rendered to Botany by associating himself with Lamarck in the publication of the third edition, may be easily estimated by this circumstance alone, that whereas the preceding Floras of France contained an enumeration of only 2700 plants, he had augmented the number, in the third edition of this work, to no less than 4700.

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* See Memoire of M. de Lamarck.

This, however, was not all; for although, out of deference to his colleague, he retains, in the first portion of his work, the artificial method of determining a plant by the system of *dichotomy* which Lamarck had invented, he proceeds, in all the subsequent parts, to arrange them according to the principles of that natural arrangement which the great Jussieu had first reduced to a system.

In his preface to the first volume of the Flore Française, published in 1805, we find him thus contrasting the distinctive merits of the natural and artificial methods.

"The natural method," he says, "endeavours to place each individual object in the midst of those with which it possesses the greatest number of important points of resemblance; the artificial has no other end than that of enabling us to recognize each individual plant, and to isolate it from the rest of the vegetable kingdom. The former, being truly a science, will serve as an immutable foundation for anatomy and physiology to build upon; whilst the second, being a mere empirical art, may indeed offer some conveniences for practical purposes, but does nothing towards enlarging the boundaries of science, and places before us an indefinite number of arbitrary arrangements. The former, searching merely after truth, has established its foundation on the organs that are of the greatest importance to the existence of plants, without considering whether these organs are easy or difficult of observation; the second, aiming only at facility, bases its distinctions upon those which are most readily examined, and, therefore, present the greatest facilities for study."

We thus perceive, that at this early period the mind of Mons. Decandolle was impressed with those philosophical principles which his subsequent labours so materially calculated to establish and to diffuse; and that, at a time when the school of Sir J. E. Smith in England was still shackled by the trammels of the Linnæan system, this great botanist was himself taking advantage of those methods of arrangement, which, in a more mature form, he afterwards presented to the world for the guidance of others.

But I am inclined to regard it as a peculiar proof, at once of the caution and of the self-control which formed a distinguishing feature in the character of this great botanist, that, so much in advance as he appears to have been of most of his cotemporaries, he should have nevertheless abstained for so many years from the publication of any work expressly designed for the elucidation, either of the physiology of plants, or of those principles of classification of which he appears to have had so clear a conception, and should have confined himself, as it would appear, exclusively to a laborious accumulation of facts, calculated to illustrate and to confirm his principles, before he indulged himself in a fuller development of them.

From the period at which he became associated with Lamarck in the publication of the Flore Française, till the year 1812, he was employed almost incessantly in studying the details of the botany and agriculture of France; and in the course of that time, as he himself assures us, traversed the whole of that extensive country, herborising in every province, and presenting each year to the Government a report, embodying the results of his labours and researches during the preceding summer.

Nor could he have chosen a better method for at once enlarging his views of nature, and putting to the test the truths of his preconceived views; the compilation of a local Flora, indeed, may only be serviceable in disciplining the mind to habits of accurate observation, but the survey of a country so large as France then was, combining such an extent of geographical range, and so many differences of local position, would also expand our views of nature, by furnishing us with examples of a very large proportion of vegetable forms, specimens of the productions of a considerable variety of distinct countries.

Thus, the flora of Picardy and Normandy is analogous to that of the neighbouring coasts of England, or of the Netherlands, that of the centre of France approaches, in the character of its vegetation, to the south of Germany, and that of Languedoc to the north of Spain; whilst the neighbourhood of Toulon and of Hyères partakes even of the climate of southern Italy—for the orange and the date, which thrive along many parts of the Gulf of Genoa, do not reappear till we reach a latitude somewhat more southern than that of Rome.

And whereas the Alps of Dauphiny and the Pyrenees exhibit the influence upon vegetation of an atmosphere rarified by the elevated nature of their position, the long extent of the coast may enable us to contrast the productions of a climate modified by the effect of the sea, with that which belongs more peculiarly to the interior of continents.

It was not till after the completion of this great work, when his authority, as an accurate, as well as a profound botanist, had been established throughout Europe, both by the estimation in which his publications were held, and also by the reputation of the lectures he delivered at Montpellier, where, in 1810, he had been appointed professor of botany to the University, that he ventured upon that admirable Treatise, which was intended, at once to establish a *code* of *laws* for directing future botanists in their description and arrangement of the species of plants, and to explain the philosophical principles upon which such laws were to be justified.

It is far from my intention to ascribe to Mons. Decandolle the sole merit of the views which he promulgated in the work alluded to, for of all men certainly he is the one who least requires from his biographer the sacrifice of the reputation of other philosophers, to enhance the glory of his own.

Linnæus himself, indeed, had expressed in the strongest terms his sense of the importance of a natural classification, and had thrown together the greater part of the then known genera of plants in groups or families, designated by their appropriate names, though without defining the characters of the latter.

Bernard de Jussieu, in France, had also exemplified this method, by his arrangement of the plants in the royal garden at Trianon, although he did not reduce to writing the principles on which he had proceeded.

Adanson had gone somewhat further, by labouring to establish the necessity of founding a system of classification, not on one, but on all the organs of a plant collectively; but he too stopped short of the mark, by not sufficiently appreciating the relative importance of the several organs, thus placing them all, as it were, upon the same level, and estimating the affinities between plants, by the number, and not by the importance, of their points of agreement.

Lastly, the younger Jussieu, in his important memoirs published in the years 1777 and 1778, laid down correctly the laws which were to determine the relative value of these organs, by which he afforded a clew to the principles which had guided himself and his uncle in the classification which they had adopted.

What remained then for Decandolle to achieve, was the reducing to certain fixed principles those deviations from the normal structure which are perceived in plants naturally allied —explaining how it happens, that species or genera, which approach each other so nearly in the character of those organs which Jussieu had justly considered the most important, should differ, nevertheless, both with respect to the number, and even sometimes in the entire absence, of parts in the one, which exist in the other.

In short, whilst Jussieu established the general principles of a correct classification, it remained for Decandolle to remove the difficulties which interfered with their application to particular cases.

Nor was this all—for Jussieu contented himself, with laying down those practical rules which were to guide future botanists in grouping together the several objects which present themselves in the vegetable kingdom, and with affording in his works correct models of classification for others to imitate; whilst the task which Decandolle undertook, was that of referring to their first principles the rules and practice of this school, explaining thereby the reasons on which they were founded, and vindicating the correctness of the models which they had presented for our imitation.

"The theory of a natural classification," remarks Decandolle, "has never yet been properly set down in print, even by those who have contributed most to advance it. Connected, as it is, with all branches of the science, we can only arrive at it by dint of laborious investigations and continued reflections, of which it ought, at this time of day, to be the groundwork, and not the result. Whatever we are able to learn on the subject may be reduced to certain general ideas, which botanists of an higher order have put forth, and that in their conversation, rather than in their writings, being still amongst the number of those opinions which Bacon named *floating*, because, having never been methodically expounded, they never could be seriously discussed."

Now, the principles on which a natural classification proceeds, are composed essentially of three parts. 1*st*, An estimation of the relative importance which we ought to assign to the several organs compared one with the other. 2*d*, A knowledge of the circumstances which may lead the observer astray relative to the true nature of these organs; and, 3*d*, An estimation of the importance which ought to be attributed to each of the *points of view* under which the same organ admits of being regarded.

With respect to the 1st and 3d of these,—namely, the importance of the several organs considered relatively, and the importance of the several points of view in which the same organ may be regarded,—Decandolle has done nothing more, than to reduce to a system the rules upon which Jussieu and other preceding botanists had proceeded in their natural arrangements of plants, and to explain the principles upon which their rules were founded, or by which they admit of being justified.

But, with respect to the 2d part, namely, the appreciation of the circumstances which may lead the observer astray as to the true nature of the organs themselves, he has the merit of having unfolded a theory, at once ingenious and philosophical, of the highest practical utility with reference to the details of botany, and calculated to simplify, as well as to enlarge, our ideas with respect to the organization of vegetables.

In my Inaugural Lecture on Botany I have already presented a sketch of this one of Decandolle's treatises, which, though concise, may perhaps serve as a sufficient account of it for the present occasion.

"The causes which bring about a deviation from the normal structure of a particular part, and thus lead a botanist to take a mistaken view of its nature, or at least of its structure, may be reduced to three: 1*st*, The abortion of some one or more of those organs, which, in the regular course of things, are considered as natural to it; 2*dly*, An alteration in its structure, and consequently in its functions; 3*dly*, The union or coherence of several organs, so as to appear like one.

" These causes are ranked by Decandolle under the three general heads of the abortion of organs, their degeneration, and their mutual coherence; and any one of them may be considered competent to induce such a change in the general appearance of a plant, as shall render it altogether different from another to which it would, on general grounds, appear to be closely allied.

" That particular organs in plants do frequently become abortive, in consequence of the common accidents of excessive or defective humidity, light, &c., had been before admitted; but to Monsieur Decandolle we are indebted for assigning a wider influence to this cause, and for shewing, that in many cases there are forces in regular operation which produce a constant alteration *in*, or obliteration *of*, certain parts.

" If, indeed, we admit, that such effects may and do arise from internal as well as from external causes, from the effect of the mere growth and development of parts connected with its own structure, as well as from the operation of foreign agents, it is plain that they would extend, not to a few only, but to all the individuals belonging to the family of plants possessing the kind of structure which occasions it.

" Thus, for example, we observe in the horse-chesnut three seed-vessels or carpels, each containing two seeds; whilst in the fruit we perceive in all never more than three seeds, and sometimes only a single one. It is evident, therefore, that at least three of the seeds have died away, not from any cause which can be considered accidental, but from something inherent in the very structure of the tree. We may indeed trace the gradual decay of these abortive seeds, by opening the seed-vessel at different stages of its growth. In like manner it is found to be the rule, that in some cases the terminal, in others the lateral buds, will arrive at maturity; but, that the abortion of the one arises merely from the development of the other, and not from any inherent peculiarity of structure in itself, has been proved, by removing the bud, which commonly expands at an early age, by which means the one which is commonly abortive is made to develope itself, and to arrive at maturity.

" The reality of this occurrence cannot therefore be questioned, but to pronounce in what cases it has actually happened, becomes a question of great intricacy.

" The first principle on which M. Decandolle proceeds, in order to determine what organs in a particular plant have become abortive, or are deficient, is by observing what are called the monstrosities to which the species is liable, or its occasional deviations from the accustomed standard.

" These monstrosities arise in some cases from a return to he primitive type of the species, in consequence of the removal, by accident, of those forces which usually modify its natural condition.

" In the horse-chesnut, for example, the six embryos rarely ever grow to maturity, because those which first have acquired vitality abstract nourishment from the rest, and thus cause them to die away.

" It might happen, however, by some singular accident, that all the six embryos received the principle of life at one and the same instant of time, on which supposition the existence of six mature seeds in the two seed-vessels might occur—a monstrosity which, so far from being a further departure from the natural form, would be in fact a return to it.

" The second method, by which the same point is determined, consists in examining the general analogy subsisting between the plant and others. If, for instance, all those species, which bear the nearest resemblance to the one we are examining, should have five stamens, whilst this possesses only four, we might reasonably conclude, knowing the great tendency of this organ to become abortive, that one habitually dies away, owing to some cause incident to the nature of the vegetable.

" The abortions which take place, may occur either from the plant being nourished in excess, or defectively. By an excess of nourishment, the growth of the contiguous organs may be so accelerated, that the part itself is prevented growing, or becomes stunted; by defect of nourishment, on the contrary, the same consequence may directly ensue, and under either state of things one of two results will occur, either that the organ is so diminished, as to be incapable of performing its proper office, or that it is entirely obliterated. In the former case it often happens, by a beautiful provision of nature, that it is transformed into some other organ, and discharges certain other functions. Thus branches, petioles of leaves, petals of flowers, and other parts, degenerate, sometimes into thorns, and at other times into tendrils; thus the branches, becoming succulent, acquire the appearance, and perform the functions, of leaves; thus that which is essentially nothing more than one of the envelopes of the kernel of the peach, becoming pulpy, is converted into a wholesome kind of fruit.

"The third cause of deviation from the accustomed standard is the mutual adhesion of certain parts, a process similar to that which we produce artificially in the operation of grafting, and which often takes place also under natural circumstances.

"It is, therefore, quite intelligible that this same union of parts should also be produced in consequence of their natural proximity. Thus, if two ovaries grow very near each other, it is obvious that they will have a tendency to cohere. M. Decandolle, therefore, contends, that the corolla and the calyx are in fact compound organs, made up of a certain number of petals and of sepals which have grown together, that a seed-vessel is a congeries of as many distinct organs as there are cells, and that a flower is no assemblage of individuals clustered round a common centre."

The sagacity of our countryman, Robert Brown, had already led him to point out this principle, so far as relates to one portion of the subject, for in his Prodromus Floræ Novæ Hollandiæ, published so long ago as 1810, he pronounces, that all multilocular capsules are composed of a number of thecæ equal in number to the divisions of which they consist, and differ from each other only in the degrees and modes of their cohesion or separation.

He also, in his observations on the "Natural Family called Compositæ," published in the Linnæan Transactions for 1816, *between* the publication of the first and second editions of

Decandolle's Theorie Elementaire, announces the same truth in more clear and distinct language, stating, that he considers the pistillum, or female organ, of all phænogamous plants, to be formed on the same plan, of which a polyspermous legumen, or folliculus, whose seeds are disposed in a double series, may be taken as a type. "A circular series of these pistilla," he continues, "disposed round an imaginary axis, and whose number corresponds with that of the parts of the calyx or corolla, enters into my notion of a flower complete in all its parts."

Other hints of the same kind thrown out in this memoir, and likewise in his Appendix to Flinders' Voyages, published in 1814, respecting the family Euphorbiaceæ, shew, that the doctrine of abortion, which Decandolle has explained so luminously, was present also to the mind of Robert Brown, and render it probable, that, in the conception of some parts of the work alluded to, its author may have derived assistance from the writings of our countryman.

The Memoirs of Cassini on the Compositæ might also have improved and enlarged, though, as they were brought out in 1814, they could not have originated the ideas of M. Decandolle; but the two sources to which he seems to have been peculiarly indebted for the general views, and for the train of thought which he has put forth, were, 1*st*, The system of crystallography which had lately been developed by the Abbé Hauy; and, 2*dly*, The opinions and speculations of Mons. Lamarck concerning the successive progression of organized beings.

The Abbé Hauy had shewn, how a number of secondary forms may be produced by the same mineral species, owing to an assemblage of crystals possessing the same figure being piled up one upon the other in a decreasing series.

Thus an octohedral figure may be produced by a mineral whose primitive form is a cube, in consequence of the number of little crystals which go to constitute the aggregate which we see, decreasing in regular proportion from the sides to the centre.

This principle suggested to Mons. Decandolle the analogous idea of regarding the apparent irregularities of structure, which are seen in species of plants belonging to the same common type, as modifications produced by the causes above assigned, just as the apparent irregularity of figure which we observe in the same mineral had been referred by Hauy to certain crystalline laws acting upon molecules possessing the same type.

Moreover, a similar difference exists between the mode of considering the organs of plants adopted by Decandolle, and by antecedent botanists, as that which prevails between the system of crystallography invented by the Abbé Hauy, and that previously proposed by Romé de L'Isle.

According to the latter, each crystal was viewed as in itself a whole, possessing a certain definite figure, which was in many cases modified by truncation, that is, by having its angles bevelled off.

According to the former, a crystal is an aggregate of a number of molecules, possessing a particular figure, which, clustering together in obedience to certain laws, produce a variety of secondary forms, all, however, bearing some relation to the primary one.

So, according to the old mode of considering plants, the corolla, the calyx, the seed-vessel, &c., was each considered a simple organ, and the petals, the sepals, the carpels, &c. its parts—whereas Decandolle regards each of the former as a compound organ, and the latter to bear the same relation to it, which the primitive molecules in Hauy's system do to the crystals formed by their union.

But the individual, to whom probably Decandolle was most indebted for the germs of those opinions, which he has so ably developed in his Théorie Elémentaire, was his colleague and associate, Lamarck; and I could hardly fix upon any circumstance in the whole of his scientific career, more calculated to exalt his character morally as well as intellectually, than the use he has made of the ingenious but fanciful views which he obtained from this source, and the discrimination which he exercised in separating the pure metal from the base alloy.

It is foreign to the objects of this Society to enter upon *any discussions* connected with religion, nor indeed, if I were to allude to that part of M. Decandolle's character, should I be able to do justice to him in these respects, not having been honoured with a sufficient degree of intimacy with him in the privacy of his domestic circle, to learn his sentiments on those grave subjects.

This, however, I may venture to assert, that whilst there is no passage in any of his numerous works, which can even by implication convey an impression of another kind, there are many which evince a disposition, on his part, to apply, on every suitable opportunity, the truths of his favourite science to the advocacy of the eternal interests of mankind.

The use which he and Lamarck have made of the doctrine of rudimentary organs common to them both will serve to illustrate this fact, and evince, not only the greater soundness of M. Decandolle's judgment, but likewise the moral truth, that food and poison may be extracted out of the very same materials, according to the character of the recipient.

The doctrine of rudimentary organs, that is, the notion "that parts which exercise some important function in the organization of animals or of vegetables, may exist in some species in so imperfect a condition, as to be apparently of no use to the individual," is one that scarcely can admit of dispute from those who take a wide survey of either of the two kingdoms of nature.

The mammæ of male animals in general, the stumps of wings in birds, which, like the penguin, are unable to fly, the eyes covered with skin belonging to the mole and the Proteus anguinus, and the rudiments of toes concealed under the skin of ruminant animals, are all familiar illustrations of this position.

But in the use which has been severally made of the above principle, the genius of the two philosophers alluded to stands remarkably contrasted.

By Lamarck it was regarded as a confirmation of that extravagant hypothesis of appetencies creating parts, by which, though without directly denying the existence of a Deity, he represented his agency as being as little exercised in the works of creation, as that of the gods of Olympus were according to the system of Epicurus.

Out of deference for the opinions of his fellow men, or perhaps from some latent sentiment of religion at variance with his philosophical dogmas, he admitted, that the order of nature emanated from the Deity, but supposed that it proceeded to do its work, by blind and imperfect, and merely mechanical efforts, productive at first of only rough and abortive draughts of what, in the course of an infinite succession of ages, ripened itself into its present wonderful complexity, and perfection of form and structure.

So even Epicurus, out of respect for the common opinions of mankind, the innate ideas, as it were, which existed in the minds of others, admitted that there were gods, but removed them from all share in the concerns of humanity, by supposing the whole structure of the universe to result from a fortuitous concourse of atoms.

How different in these respects was the proceeding of M. Decandolle!

He did not indeed attempt to deny the existence of rudimentary organs, from seeing the use which others had made of the doctrine—to have attempted this indeed would have been as hopeless a task, as to deny the deductions arrived at by geologists with respect to the age of the world, because some persons may have perversely availed themselves of such facts as a handle against revelation—but, boldly admitting their reality, and skilfully availing himself of this principle as a clew whereby to trace the affinities between plants, he vindicated it from the imputation of being in any degree inconsistent with the existence of design, or of lending any countenance to the doubts of the sceptic.

According to his views, all organized beings, when compared one with another, present groups of greater or lesser extent, which themselves form parts of groups embracing a still wider range, and are divisible into others of a subordinate description. Each group is subject to two classes of laws; the first producing that regular order in which its organs are disposed, or in other words the symmetry of its organization; the second regulating the action of the processes of vitality, from which often results such a degree of derangement in the symmetry of its parts, that their natural disposition may thereby be completely disguised.

This derangement of the normal structure may be ascribed —either to the abortion of certain organs—to their alteration in form and appearance—or to the adhesions between organs of the same or of different descriptions.

The existence, then, of rudimentary parts, is only a consequence of those general rules, which the divine Author of Nature has thought fit to impose upon himself in all the arrangements of the universe, and can in no wise be regarded as inconsistent with the idea of design, if we only can shew, that the whole proceeds upon a consistent plan, and that plan a wise one, inasmuch as each organ, in the great majority of cases, and in its perfect and developed form, is subservient to some beneficial purpose.

As a consequence, of that general analogy which runs throughout the whole of organized nature, and of the interference of causes which in their main result are productive of good, we find parts existing in a rudimentary or abortive state in one species, which in others serve some manifestly important office; neither would it be any objection to the idea of design, if it could be proved, that in this rudimentary condition they were absolutely useless, although it must be considered as an additional evidence of provision, when, as in many instances, we are able to shew, that they become subservient to a new purpose, by being unfitted to their primary one.

Thus the parts of the calyx in many composite flowers degenerate into a pappus, or down, which, being of a light and feathery texture, serves to waft the seeds attached to it to a great distance, and in this manner to disseminate the species; thus the nectaries, which are regarded as degenerated stamens, secrete honey, and by this means attract insects, by whose entrance into the flower, the pollen is dispersed and lodged upon the pistils.

Perhaps, had not one of the seed-vessels of leguminous plants been constantly abortive, the seeds would have all been so stunted in their growth, as to have been unfitted for supplying nutriment to animals.

These, and other facts that might be alleged, prove, that the degeneration or abortion of particular organs, often serves some wise purpose with reference to the plant itself, or to *other beings; and that* the same may be the case in other instances, in which we do not perceive it, it would be presumptuous to deny.

Nevertheless, it does not seem requisite for the argument as to final causes, to contend, that every organ must have a definite use in all the individuals in which it occurs, since its existence may be regarded, as being nothing more than a consequence of that general law of nature above stated, the wisdom of which there is no ground for impugning.

" If," says M. Decandolle,* " on a subject so grave and so elevated, I may be permitted to avail myself of a comparison somewhat mean and trivial, I shall perhaps render my views on this subject somewhat better understood.

" I will suppose myself seated at a splendid banquet, and certainly the repast which Nature sets before us may well merit this appellation,

" I endeavour to discover what evidence can be afforded that this banquet is not the result of chance, but has been due to the will of an intelligent being. No doubt, I should remark, that each of the dishes is in itself well prepared (this is the argument of the anatomist), and that the selection of them implies a reference to the wants of the individuals who partake of them. (This is the reasoning of the physiologist.) But may I not likewise observe, that the dishes that constitute this repast are arranged in a certain symmetrical order, such as is agreeable to the eye, and in itself announces design and volition ?

" Now, if on examining the above arrangement, I should find certain dishes repeated, as for instance in double rows, for no other apparent reason, than that the one might in a manner correspond to the other ; or observe, that the places which they should occupy were filled with imitations of the real dishes, which seem of no use with reference to the object of the repast, ought I, on that account, to reject the idea of design ?

" So far from this, I might infer from the very circumstances stated, an attention to symmetrical arrangement, and consequently the operation of intelligence.

"Now this is precisely what happens on the great scale in nature. Considerations derived from the symmetry of parts correct in great measure what is deficient in the theory of final causes, and tend, not only to resolve many difficulties, which present themselves in the general economy of nature, but even to transform them into evidences of the existence of this very order."

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* Théorie Elémentaire. 2d edition, page 185.

And here, perhaps, I may be permitted to make a short digression, in order to say a few words with respect to the general spirit and influence of the writings which have proceeded from the Republic of Geneva.

Let others, if they please, censure the laxity of opinion which is attributed to their theologians—my more grateful as well as more appropriate office in this place shall be, to bear testimony to the general moral tone, and beneficial tendency of their literature.

Had it not been for the existence of this independent focus of learning and talent, all French publications would have been but a reflexion of the light which radiated from the often corrupt atmosphere of Paris; for in France everything centres in the metropolis, and in that country, as a witty writer* has quaintly expressed himself—the opinions of the provinces are of little more importance *than the opinion of a man's legs.*†

But Geneva, from its high intellectual eminence, its Protestantism, and its independent political position, has always possessed a school, both of literature and science, exclusively its own, so that not only those of her sons who have continued under her wing during life, but even the offsets she has sent forth to other lands, have preserved the impress of those national characteristics which they had acquired from early education.

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* Heyne.

† M. Flourens has unexpectedly supplied me, in his Eloge of Decandolle, with an anecdote which may serve to confirm this position. When Decandolle had been appointed by M. Cretet, the Minister of the Interior, to his professorship at Montpellier, the following conversation passed between the minister and Laplace, who, by way of expressing his high admiration of Decandolle, began it as follows:—" Monseigneur, vous nous jouez un mauvais tour, nous comptions avoir bientôt M. de Candolle, à l'Institut." "Votre Institut! votre Institut! s'écrie M. Cretet." "Eh quoi!" repond M. de Laplace, tout etonné. " Savez vous que j'ai quelquefois envie de faire tirer un coup de canon sur votre Institut? Oui, monsieur, un coup de canon, pour en disperser les membres dans toute la France. N'est ce pas une chose deplorable de voir toutes les lumières concentrées dans Paris, et les provinces en ignorance. *J'envoie M. de Candolle* à Montpellier, pour y porter l'activité."

Thus Necker maintained, even in his financial measures at Paris, the ideas that he has brought with him from Geneva; and his illustrious daughter was reproached and almost proscribed by Napoleon, for the singular reason, that her writings were not written in a French spirit.

Nor will an impartial critic deny, that the literature of Geneva, whatever may be its faults, possesses a greater purity and elevation of sentiment, than belongs to the school which was at one time regarded as essentially Parisian. With one lamentable exception, no doubt, which we regret the more, because the gross impurities that sully the works to which I allude, are perceived to have been the offspring of a mind, not destitute of "some glorious elements," * or deficient in high and noble aspirations, the writers who have emanated from the little Republic of which I speak, may fairly participate in the praise which the most eminent of her native historians† claims for himself as his highest merit, namely, "that of never noticing vice but with the disgust it deserves, never surrounding it with seductive pictures, or treating it as a subject of pleasantry; and, in the course of the whole of his voluminous publications, of having never written a single passage which a modest female might not read aloud without a blush."

As for Decandolle, he partook fully in that sentiment of nationality which has kept Geneva distinct from Paris, in science and literature, as well as in government.

It is related of him, that when, in 1809, he represented the department of Leman in the Assembly of Notables, convened by Bonaparte as Emperor, on being presented to the latter, and asked by him how Geneva was pleased with its union with France, he had the courage to remain silent; and no sooner had the peace of 1814 secured to his native place an

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* "A goodly frame of glorious elements,
Had they been wisely mingled."

† See Sismondi's Preface to his "Histoire des Francais."

independent existence, than he gave up his emoluments at Montpellier, and preferred the almost honorary appointment which he henceforth discharged as Professor of Natural History at Geneva, to any more lucrative office in a foreign city.

From this period may be dated the commencement of those important works, upon which his reputation amongst European botanists is principally founded.

In 1818 appeared the first volume of his Systema Naturale, intended to embrace a detailed description of all known plants, arranged according to their natural affinities or design,—an undertaking which, since the days of Ray, no botanist had had the courage to attempt.

He was not, indeed, unaware of the magnitude and difficulty of such a work, or of the danger lest his labours should be subverted by discoveries made during their progress ; but he was encouraged to proceed in it, by the consciousness that a treatise of this description, even though imperfect, would be the one of all others most instrumental in spreading a knowledge both of general and special botany.

It is indeed a happy circumstance for the cause of science, when an individual, possessing the comprehensive views and the powers of generalisation which belonged to Decandolle, can be induced to enter upon this species of labour ; and not one of the least advantages accruing from it I conceive to be, that it relieves the pursuit itself from the imputation of frivolousness, to be found worthy of occupying so large a portion of the attention of one, who had already shewn himself, by his previous publications, capable of grappling with the more philosophical departments of the science.

It may be remarked, that whilst in the Flore Française, and I believe in most other works of antecedent date, founded on the natural system, plants of the most simple structure were placed first, and the more complex ones afterwards, the contrary order has been pursued in the Systema Naturæ of Decandolle.

And in this difference of arrangement I think I can trace the influence of those general views which he had adopted in opposition to his distinguished colleague and early master, Lamarck.

It was, no doubt, quite natural and consistent in the latter, imagining, as he did, that the more complicated forms of vegetable life had proceeded out of the simpler ones, by a number of successive tentative efforts of creative energy, to imagine that he was following the order of nature in describing, in the first place, those plants which he conceived to be of earliest production ; whilst Decandolle, who regarded the whole vegetable kingdom as equally the result of the same wise and beneficial plan, and who had been taught by the researches of Cuvier, that the inhabitants of the early periods of the world were as complicated in their organisation, and as skilfully contrived for their respective uses, as those at present in existence, was led to prefer that mode of considering the subject, which enabled him to place first before his readers the organs of a plant, in their most complete state of development, and therefore in their most intelligible point of view.

He felt, that it was pursuing a mistaken analogy, to imagine that the organs of reproduction or of vegetation could be studied with more facility in a moss, than in a flower ; it might be rather said, that in the former they were in a manner in a rudimentary condition, and consequently that their true uses could best be inferred by analogy, after we had fully examined them in plants of a more complicated structure ; just as we should be at a loss to explain the uses of the eye, from examining it in the mole, or of the mammæ from a dissection of those in the male subject, instead of beginning with those cases in which the above organs were in a state of the most complete development.

Decandolle accordingly commences his system with the family Ranunculaceæ, as that in which the natural symmetry of plants belonging to the Dicotyledonous division is in the least degree departed from, the sepals, petals, stamens, and even the pistils, being here separate and distinct; and he then proceeds, step by step, to trace the different degrees and kinds of irregularity which may be perceived in those other natural families which he places before us in succession.

Nor are the more technical, or, as it may be termed, the echanical arrangements adopted in this treatise, selected with ss judgment and discretion.

In the Systema Naturæ, the authority for each description is scrupulously given; and it is stated, by appropriate marks, whether the plant has been observed by Decandolle himself in a dry or in a living state, cultivated or wild. The synonymes of each species are appended, with a mark affixed to the name of their author, whenever the identification has been fully made out by an actual comparison of the specimen referred to with that on which Decandolle's description is based.

The *habitat* is given with greater accuracy than heretofore, by appending to it the name of the author on whose authority it rests, either in italics, where Decandolle himself has seen the specimen referred to, or in roman letters, included in a parenthesis, where he has not; whilst, where it rests on Decandolle's personal examination, the locality is given without any name at all.

Another point attended to scrupulously in this treatise was the breaking up of the genera into natural sections, so as to group the species together, as much as possible, according to their natural affinities; an idea which has been followed out by subsequent botanists, with regard to the natural families themselves, which are now arranged according to their alliances, and thus serve as links whereby to connect in one consecutive chain the most general divisions into classes, with the most subordinate one into species and varieties.

But even the indefatigable zeal and the steady perseverance of Mons. Decandolle were found unequal to the herculean task of describing, in the detailed manner originally proposed, the enormous catalogue of plants at present enumerated, swelled, as it has been, by the researches of modern botanists, from 8000 species known to Linnæus, to more than 50,000; and, accordingly, after bringing out two volumes of his Systema, embracing within their compass 11 natural families, he determined on carrying on his work in a more compendious form, under the title of *Prodromus Systematis Naturalis.*

What the extent of his original work would have been, had it ever been completed in its original plan, may be estimated from this calculation alone.

The Prodromus, at the time of his death, consisted of six thickly printed volumes, each averaging about 700 pages, and of a seventh of half that size, and yet it includes only 102 natural families; whereas the whole number comprehended in his son's enumeration of those belonging to the class of flowering plants is 195.

It is true, that one of those completed is the immense order of Compositæ, which alone has been estimated at nearly a quarter of the whole of the Dicotyledonous division; but then, on the other hand, it must be recollected, that, during the interval since the work commenced, such vast additions have been made to the catalogue of plants, that the families hereafter to be described would be more voluminous in proportion than the earlier ones.

We may, therefore, perhaps calculate, that the Prodromus, had it been completed, would have formed 15 volumes of 700 pages each; but the plants described in the two volumes of the Systema are compressed into 236 pages of the latter work, so that the Systema, if executed on the same plan, would have occupied no less than 44 volumes octavo.

For, if 236 pages = 1 vol. — 10,500 (viz. 15 vols. of 700 pages each) = 44 vols.

This great undertaking, commencing with the preparation of the first volume of the Systema, which was published in 1818, occupied him till his death, which occurred in 1841; but the last portion of it which appeared was the concluding part of the description of the Compositæ, bearing the date of 1838.

We must not, however, suppose, that the whole business of his life during so long a period consisted in the exhausting labour of describing and classifying species. From time to time, for instance, during this interval he brought out those admirable Monographs, in which he has delineated in so masterly a manner the general characters of particular natural families.

These Monographs were intended to serve as fuller explanations of the grounds of that classification which he had adopted in his Prodromus, as illustrations of those principles which he had laid down in his Theorie Elementaire, and as criticisms on the plans of arrangement which had been proposed by antecedent writers.

They hold an intermediate place between the mere particular descriptions of species which are contained in the Prodromus, and the general observations on the structure of plants considered in the aggregate, which are found in the Organographie; constituting the groundwork of the former, and the data upon which the latter was constructed.

Thus, in his Memoir on the Cruciferæ, he carries us in detail through the structure of all the parts, first, of vegetation, and afterwards of reproduction, belonging to this important natural family; and he shews, that the only distinction which can be relied on for separating its members into natural groups, are drawn, either from the form of the embryo, or from that of the seed-vessel. If we adopt the former as the basis of our system, we shall divide the Cruciferæ into five natural groups, according to the position of the Radicle with reference to the Cotyledons; if we adopt the latter, we shall distinguish them into six, according to the position of the valves of the Seed-vessel.

This latter method he shews to be preferable to the old Linnæan division, depending upon the length of the pod, as the latter admits of no exact limits, and as it places together genera in no way allied, and divides others which are naturally connected; but he nevertheless regards it as of inferior moment to the distinction founded upon the embryo, both because the latter is an organ of greater importance than the seed-vessel, and because there is not such a gradation in its form, as is found in that of the pod which incloses it.

He adopts, therefore, as the basis of his classification, the principle suggested by Robert Brown, with respect to the manner in which the radicle is folded upon the cotyledons, and afterwards subdivides the groups so formed according to the form and mode of opening of the seed-vessel.

He thus, by means of these two characters, constructs twenty-one natural groups, and satisfies himself of the correctness of the principles upon which he has proceeded in his classification, by finding that the genera thrown together by virtue of this arrangement, are really such as stand most nearly allied one to the other.

Thus, as in the physical sciences, we commence by making

Tabular Vew of the Cruciferæ, distributed according to their Cotyledons and Seed-Vessels.

SEED-VESSELS.	COTYLEDONS.				
	ACCUMBENT.	INCUMBENT.			
	FLAT. (o =) PLEURORHIZÆ.	FLAT. (o II) NOTORRHIZÆ.	ONCE DOUBLED. (o >>) ORTHOPLOCÆ.	SPIRAL. (o II II) SPIRALOBEÆ.	TWICE DOUBLED. (o II II II) DYPLICOLOBEÆ.
SILIQUOSE. — Valves opening longitudinally, partition, linear, elongated, wider than the seeds.	ARABIDEÆ. — ARABIS, and 13 other genera.	SISYMBRIEÆ. — SISYMBRIUM, and 6 other genera.	BRASSICEÆ. — BRASSICA, and 3 other genera.		HELIOPHILEÆ. — HELIOPHILA, &c.
LATISEPT. — Valves opening longitudinally, partition oval or oblong, valves flat or convex.	ALYSSINEÆ. — ALYSSUM, and 15 other genera.	CAMELINEÆ. — CAMELINA, and 3 other genera.	VELLEÆ. — VELLA, and 3 other genera.		SUBULARIEÆ. — SUBULARIA.
ANGUSTISEPT SILICULOSE. — Valves opening longitudinally, folded, partition very narrow	THLASPIDEÆ. — THLASPI, and 8 other genera.	LEPIDINEÆ. — LEPIDIUM, and 4 other genera.	PSYCHINEÆ. — PSYCHINE and SCHOUWIA.		BRACHYCARPEÆ. — BRACHYCARPA.
NUCAMENTACEOUS. — Valves indistinct or indehiscent.	EUCLIDIÆ. — EUCLIDIUM, and 2 other genera.	ISATIDEÆ. — ISATIS, and 3 other genera.	ZILLEÆ. — ZILLA, and 2 other genera.	BUNIADEÆ. — BUNIAS.	
SEPTULATE. — Valves opening longitudinally, furnished with transverse partitions in their interior.	ANASTATICEÆ. — ANASTATICA, MORETTIA.				
LOMENTACEOUS. — Dividing transversely.	CAKILINEÆ. — CAKILE, and 3 other genera.	ANCHONIEÆ. — ANCHONIUM, and 2 other genera.	RAPHANEÆ. — RAPHANUS, and 3 other genera.	ERUCARIEÆ. — ERUCARIA.	

experiments of a kind more or less exact, and having, by means of them, traced our route, rectify it by means of geometrical formulæ, which serve as a check upon the errors of manipulation ; so, after having thrown together all known plants of the cruciform family into groups determined by the sum of their affinities, he submits these groups to the rules of theory, and finding the two methods to lead him to the same result, is satisfied that he has made a near approximation to truth.

But the object of the classifier, is not only to arrange in the most natural order the several genera belonging to the family which he describes, but also to ascertain the relations of the family itself taken collectively to other portions of the system.

From the earliest times at which botany was studied, the importance of this inquiry has been felt, and the first attempts made to effect this object consisted in disposing the several objects in a linear series, according to the degrees of their deviation from some one taken as a standard of comparison.

The objection to this method was, that the same plant might be nearly allied to one member of the series in certain respects, and to one occupying a very different place in others ; and Linnæus himself, being fully alive to the impossibility of constructing a linear arrangement which should harmonize with nature, suggested the ingenious idea of representing the relation in which plants stand one towards each other, by a kind of map, in which the classes should stand toward each other, as the quarters of the world do on an artificial globe, including the families as separate countries, their genera as provinces, their species as districts. As on the map, each district touches many others, at distinct points, so likewise do the families in the vegetable kingdom ; as the individuals in each district agree in so many particulars with the inhabitants of the neighbouring districts as to be scarcely recognisable from them, so also do the species composing various allied genera.*

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* Giesecke, in his " Prelectiones in Ordines Naturales Plantarum, Hamburgh 1792," has attempted to carry out this idea, in a Chart, entitled Tabula Genealogico-*Geographica Affinitatum* Plantarum.

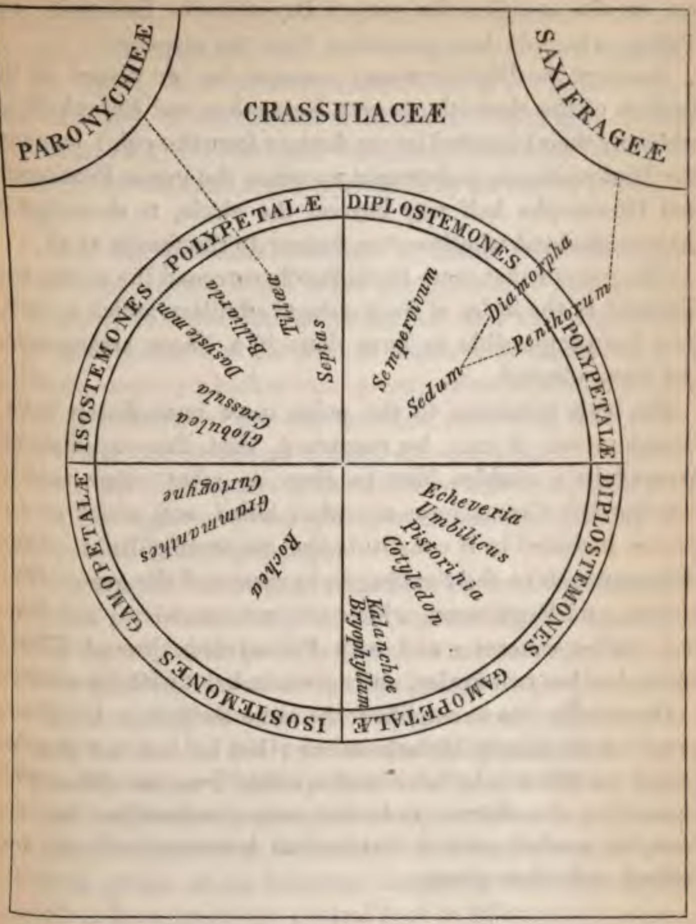
Decandolle, in his Théorie Elémentaire, and other writings, has followed up this notion in his usual masterly manner; but aware that a complete map of this kind can only be constructed when the whole vegetable kingdom is fully investigated, just as it would be impossible to make a general map of the globe, till every part of it had been explored, he has contented himself with the preliminary labour of constructing local maps, as it were, of different countries, by pointing out the relations of each of the families which he notices to the rest.

In some cases, indeed, the characters of a family are so well marked, and so distinct from those of most others, that it is difficult to say to what the plants included in it are most allied. They stand in the relation of islands in the midst of a vast ocean, "penitus toto divisos orbe," and in any map of the vegetable kingdom which may hereafter be constructed would be represented as detached groups.

Such is the case with the Cruciferæ, a family which, although it bears some analogies to the Papaveraceæ in the number of its petals, and in the structure of its fruit, possesses nevertheless so many peculiarities of structure, that it seems to lie apart from all other natural groups, except, perhaps, from the small one of the Capparideæ.

But in the greater number of instances, the characters of the natural groups graduate into those of several others, like the countries included within the same continent; and in these cases, Decandolle has presented us with a sort of pictorial view of the entire family, representing the several genera of which it consists diverging from a common centre, around the circumference of which are placed the families with which these genera bear respectively the nearest connection.

Thus in the case of the Crassulaceæ, the division into tribes is drawn from the relation of the stamens, in point of number, to the petals, they being in some cases the same, in others double of the latter; and these two tribes are again subdivided, according as the petals are united or distinct.



We, therefore, obtain four groups, viz. :—

Isostemones polypetalæ.	Diplostemones polypetalæ.
Isostemones gamopetalæ.	Diplostemones gamopetalæ.

These distinctions Decandolle delineates by the device of a circle, for the whole family taken collectively, divided into four equal parts, to represent the four tribes alluded to, whilst the genera placed circularly nearest to the central point, are those which shew the general characters of the family in the most perfect manner, and in each division those in the borders indicate the genera which are more anomalous.

Thus in the division entitled Isostemones polypetalæ, we see on the margins the genera Dasystemon, Bulliarda, and Tillæa, which deviate somewhat from the standard.

Amongst the Diplostemones gamopetalæ, are placed on the borders of the circle the genera Kalanchoe and Bryophyllum, which by their pinnated leaves deviate from the rule; amongst the Diplostemones polypetalæ we place the genus Penthorum and Diamorpha half way beyond the circle, to shew that it may be doubted whether they belong to the family at all.

The genera that more immediately surround the centre, are disposed in the order of their natural affinities, which it would have been impossible to have done if a linear arrangement had been adopted.

But with reference to the point more immediately under consideration, it may be remarked, that this expedient of Decandolle's enables him to shew to what other natural families the Crassulaceæ are most allied, and which of the genera included in it constitute the connecting links. Thus they approach to the Saxifrageæ by means of the genera Diamorpha and Penthorum, which are not succulent, and have the ovaries concrete, and with Paronychiæ through Tillæa, which has but two ovules, and agrees in habit with Illecebrum.

Decandolle has exemplified the same method in his Monograph of the family Melastomaceæ; but he has not thought proper to follow it in later works, either from the difficulty of presenting the characters in the same compendious form, or from the marked natural distinctions between the family described and other groups.

One subject particularly attended to by Decandolle in all these monographs was, the relative proportion of the members of each family found in different regions of the globe. By ascertaining in each instance this point, he hoped to obtain, at length, the requisite data for perfecting a branch of botanical science, in which, from a very early period of his life, he had taken a lively interest—I mean the geographical distribution of plants.

Already, in the year 1807, he had given, in his Flore Française, a sketch of the several botanical regions into which France might be divided; and when, in 1817, Humboldt published his Prolegomena on the Geographical distribution of

Plants, and likewise, in the Memoires de la Société d'Arcueil, his masterly dissertation on the Distribution of Heat throughout the Globe, M. Decandolle contributed to the same work, a general sketch of the subject of Botanical Geography, which, in an expanded and corrected form, has been inserted in the Dictionnaire des Sciences Naturelles, by Levrault, published in 1820.

In this essay, he first considers in detail, the influence of the different agents which affect the growth of plants, such as temperature, light, humidity, the soil, and the atmosphere —he then points out the several stations or situations in a particular country which certain plants affect, as, for example, the neighbourhood of the sea, marshes, cultivated land, rocks, sand, forests, the dwellings of man, &c.—and, lastly, he passes in review the distribution of the different tribes of plants over the various regions of the globe, such as the tropical, the temperate, and the frigid zones.

Having thus, both from theory and from observation, estimated the degree of influence exerted by external agents upon the distribution of plants, he is in a condition to consider the interesting problem, as to whether the limited range of country, to which in a state of nature each species would appear to be confined, is referable solely to the above circumstances.

If the affirmative of this proposition be granted, we have then the choice of several suppositions, for either we may suppose, that plants were at first scattered indiscriminately over the whole globe, or, as Linnæus imagined, that they spread themselves from some one central spot in different directions, according as the conditions were favourable or otherwise; or, if we chose to give in to the Lamarckian view of the gradual progression of more perfect from less perfect forms, we might imagine each plant to be generated at or near the spot where it is found, owing to the influence of external causes operating upon matter endued with some principle of vitality.

But if the negative must be concluded—if, after making all allowance for circumstances, there remains something yet to be explained with respect to the geographical distribution of vegetables, we are then led to the conclusion, that each

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species was originally created within a certain distance of the locality where it is met with, having spread itself to a point more or less remote from its original site, according as circumstances were more or less favourable to its propagation.

This question, which involves higher considerations than those relating merely to botany, has been discussed by Monsieur Decandolle with his usual sagacity and judgment, and the result he has arrived at seems to be—that it is far more easy to explain, on physical principles, the occurrence in a comparatively small number of cases of the same species in distant regions of the globe, than the limitation of the greater number to certain fixed geographical limits by considerations of climate.

Thus, he observes, it would not be difficult to pitch upon two points of the globe situated respectively, either in the United States and in Europe, or in America and tropical Africa, presenting the same circumstances of temperature, elevation, soil, and humidity, and yet with a perfectly distinct flora; whilst, on the other hand, if a plant be pointed out which occurs in two localities very remote one from the other, we may generally discover something, in the nature of its seed-vessel, in its known properties, or in its uses, which may have caused its propagation and naturalization in regions to which it was not indigenous.

He thus arrives at a conclusion, in entire harmony with those views which I have before represented him as entertaining, with respect to the perfect condition in which all the works of nature had issued from the hand of the Creator; inferring, that each plant was at first established in some particular locality for which its habits and structure were suited, not that it was rendered what it is, by the operation of the causes which there affected it; contending, in opposition to Lamarck, for the permanency of species, and making common cause with Cuvier in opposing the absurd and dangerous theory of spontaneous generation.

One of the strongest reasons for preferring a natural arrangement of plants to an artificial one is, that the former affords us a clew to the medicinal, as well as to all the other *qualities* which may be expected to reside in a particular *species*.

We have seen, that Decandolle was, from an early period of his life, awake to this important application of botanical science to purposes of utility, for the medicinal properties of plants were selected by him as the subject of his Thesis, when candidate for the degree of Doctor in Physic at Paris in 1804.

In the year 1813, whilst still professor at Montpellier, he brought out a distinct work on the same subject, pointing out in it the physiological properties of each of the principal natural families; and of this treatise a second edition appeared in 1816.

In 1819, he published a new and corrected edition of his Théorie Elémentaire, in which he added, 1*st*, a chapter on the degeneration of organs, a particular case, it is true, of their abortion, but one deserving a distinct notice, from the peculiar character of the effects that are numbered under it. 2*dly*, a more full development of the principle laid down in his former edition, as to the effects produced on the organization of plants by the adhesion of organs. The general principle had indeed been clearly pointed out before, but the application of it, to explain, for instance, the manner in which all the different kinds of seed-vessel result from the union of distinct carpels, was first brought forward on the present occasion. 3*dly*, The chapter was added, to which allusion has been made in the former part of this sketch, vindicating the theory of abortive organs from those objections which had been raised against it, as though it militated against the idea of design, and clearly pointing out the distinctions between the views of their Author, and those of Lamarck.

It was quite natural, that a mind which had obtained, even in 1813, and still more completely in 1819, such clear views with respect to the causes of irregularity existing in the organs of plants, should have been carried forwards to the beautiful doctrine of vegetable metamorphosis, which is only a further development of the same theory.

No doubt, indeed, Linnæus himself had obtained a glimpse of this truth, as appears from his Thesis, called the Prolepsis Plantarum, sustained in the year 1760, in which, though under the influence of a mistaken and fanciful hypothesis, he nevertheless distinctly maintains, that the bracts, the calyx, the corolla, the stamens, and the pistil, are modified leaves.

The defects of this theory Dr Lindley states to be, " in its failing to account for the modifications which the pistil undergoes, and in the fanciful supposition, that the organs of fructification are prepared six years beforehand, and that their peculiar appearance is owing to the time of their development being anticipated by some unknown but ever acting cause."

The celebrated poet Goethe, in the year 1790, had the merit of presenting this theory divested of the above accompaniments, and his " Versuch die Metamorphose der Pflanzen zu erklären," is remarkable as an example of one of those happy guesses at truth, which great minds occasionally arrive at, by a rapid glance over nature, rather than by that patient investigation of particular phenomena, by which great general principles require, for the most part, to be worked out.

Coming, however, from an individual whose reputation was built upon works of poetry and imagination, his theory excited little attention amongst naturalists, until it was found to harmonize so remarkably with the principles, hinted at by Brown, and propounded more fully by Decandolle, with respect to the modifications which organs undergo from adhesion, &c.; and it at length came to be discovered, that the " degeneration of organs" and their " metamorphosis," are only different modes of regarding the same phenomenon,* and that the poet and the philosopher, though setting out from opposite directions, had in fact met at the same point.

This doctrine was more fully explained in his work on the Anatomy, or, as he proposed to call it, the Organography of plants, which was not given to the world till 1827, although its principal contents had been imparted to a numerous class of pupils in the lectures he had delivered for several preceding years.

In this admirable work, which can only be estimated at its full value by those who compare it with the treatises on botany published at the same time, as by Smith and Keith in England, by Mirbel in France, or by Sprengel in Germany, he proceeds upon the principle of tracing each organ through all its several modifications of structure in the different plants in which it occurs, and of reducing every part to its organic elements. Hence the whole of this treatise may be regarded as in some sense a development of the great doctrine of metamorphosis, which he had pointed out in his foregoing treatise—a detailed exposition of the symmetrical plan, which the parts of all plants affect, and of the causes which interfere with their regularity of form and disposition.

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* See some good remarks in Flourens' Eloge on the distinction between the views of Decandolle and Goethe on this subject.

It is this philosophical mode of considering the structure of plants, which has mainly contributed to impart to the work of Decandolle the superiority which it possesses over all antecedent, all contemporaneous treaties on botany, and which has even rendered it in many respects a model for those which have subsequently appeared.

The organs of vegetables are here set before us, not in dry detail, as separate and independent parts of the structure which they serve to make up, or even in a purely physiological point of view, as subservient to the uses of the individual of which they are a constituent; but they are treated, as links of a common chain, as portions of the same harmonious system, subject indeed to endless variations, and productive of continual diversities of form and function, but nevertheless all influenced by the same universal law of symmetry and order.

One of the greatest advantages attendant on this mode of treating the subject of botany, is the facility which it affords us of conveying a clear conception of, and in imprinting upon the memory, the numerous varieties of structure, which we adopt in describing and distinguishing individual plants.

How perplexing, for instance, is the enumeration of the different kinds of seed-vessel, according to old books on botany founded on the Linnæan method of classification! how wearisome to the mind, to burden itself with the names and definitions of a long string of objects, between which no relation or connexion of form has been pointed out to us!

But, according to Decandolle's method, all these several forms of organization are shewn to result from leaves variously modified, and adherent; and thus, whilst engaged in the interesting task of reducing all these variations of form to one common *symmetrical plan*, we are insensibly led to classify venth of half that size, and yet it includes only 102 natural families; whereas the whole number comprehended in his son's enumeration of those belonging to the class of flowering plants is 195.

It is true, that one of those completed is the immense order of Compositæ, which alone has been estimated at nearly a quarter of the whole of the Dicotyledonous division; but then, on the other hand, it must be recollected, that, during the interval since the work commenced, such vast additions have been made to the catalogue of plants, that the families hereafter to be described would be more voluminous in proportion than the earlier ones.

We may, therefore, perhaps calculate, that the Prodromus, had it been completed, would have formed 15 volumes of 700 pages each; but the plants described in the two volumes of the Systema are compressed into 236 pages of the latter work, so that the Systema, if executed on the same plan, would have occupied no less than 44 volumes octavo.

For, if 236 pages = 1 vol. — 10,500 (viz. 15 vols. of 700 pages each) = 44 vols.

This great undertaking, commencing with the preparation of the first volume of the Systema, which was published in 1818, occupied him till his death, which occurred in 1841; but the last portion of it which appeared was the concluding part of the description of the Compositæ, bearing the date of 1838.

We must not, however, suppose, that the whole business of his life during so long a period consisted in the exhausting labour of describing and classifying species. From time to time, for instance, during this interval he brought out those admirable Monographs, in which he has delineated in so masterly a manner the general characters of particular natural families.

These Monographs were intended to serve as fuller explanations of the grounds of that classification which he had adopted in his Prodromus, as illustrations of those principles which he had laid down in his Theorie Elementaire, and as criticisms on the plans of arrangement which had been proposed by antecedent writers.

They hold an intermediate place between the mere particular descriptions of species which are contained in the Prodromus, and the general observations on the structure of plants considered in the aggregate, which are found in the Organographie; constituting the groundwork of the former, and the data upon which the latter was constructed.

Thus, in his Memoir on the Cruciferæ, he carries us in detail through the structure of all the parts, first, of vegetation, and afterwards of reproduction, belonging to this important natural family; and he shews, that the only distinction which can be relied on for separating its members into natural groups, are drawn, either from the form of the embryo, or from that of the seed-vessel. If we adopt the former as the basis of our system, we shall divide the Cruciferæ into five natural groups, according to the position of the Radicle with reference to the Cotyledons; if we adopt the latter, we shall distinguish them into six, according to the position of the valves of the Seed-vessel.

This latter method he shews to be preferable to the old Linnæan division, depending upon the length of the pod, as the latter admits of no exact limits, and as it places together genera in no way allied, and divides others which are naturally connected; but he nevertheless regards it as of inferior moment to the distinction founded upon the embryo, both because the latter is an organ of greater importance than the seed-vessel, and because there is not such a gradation in its form, as is found in that of the pod which incloses it.

He adopts, therefore, as the basis of his classification, the principle suggested by Robert Brown, with respect to the manner in which the radicle is folded upon the cotyledons, and afterwards subdivides the groups so formed according to the form and mode of opening of the seed-vessel.

He thus, by means of these two characters, constructs twenty-one natural groups, and satisfies himself of the correctness of the principles upon which he has proceeded in his classification, by finding that the genera thrown together by virtue of this arrangement, are really such as stand most nearly allied one to the other.

Thus, as in the physical sciences, we commence by making neva, during the last century, have devoted especial attention.

Bonnet, it will be recollected, had observed the evolution of air from leaves whilst exposed to the sun's light, long before Priestley had been led by observations of a similar kind to his beautiful theory as to the influence of plants in purifying the atmosphere.

Senebier had examined, in much detail, the circumstances under which this evolution of gas took place, and the causes by which it was determined, to which Decandolle himself has made some additions, by shewing, that artificial light may, to a certain extent, supply the place of solar radiation,—a fact, however, which had been first pointed out by Humboldt, so early as 1793, in the aphorisms appended to his Flora Subterranea of Freyberg.*

But the younger De Saussure had distinguished himself above the rest, by investigating the influence at once of water, of air, and of the soil, on the processes of vegetation, and by introducing into this department of inquiry, a degree of accuracy, which renders his labours, even after a lapse of nearly forty years, still the best authority we can appeal to on many of the questions under discussion.

Into the consideration of questions which had exercised the genius of some of the most distinguished of his countrymen, Decandolle entered, as might be expected, with more than common interest, and his masterly chapter on this subject might, we should think, if properly studied, have removed, from the minds of certain German physiologists, the doubts they appear at a much more recent period to have entertained, with respect to the source of the carbon existing in plants, and also perhaps have induced an illustrious living chemist of that country,[*] to limit to the naturalists of his own nation the strictures, which he seems to consider applicable to the general body of those in Europe, for being, as he conceives, so far in arrear of the actual state of chemical knowledge in this respect.

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* I have been reminded by the perusal of M. Flourens' Eloge, of an omission in the former part of this memoir relative to the subject here alluded to, namely, of the fact, that these researches on artificial light were made known to the world so early as the year 1800. (See Memoires des Savants Etrangers de l'Institut, vol. i.) He there established, that the period of the sleep of plants may be gradually reversed, by keeping them in the dark during day, and exposing them to artificial light at night; and that we might communicate a green colour to etiolated leaves by the same action, although we could not produce from them a sensible evolution of oxygen. Thus concludes Flourens—

"La vie des plantes est un phenomene bien plus compliqué, bien plus rapproché de la vie des animaux qu' on ne l'avoit soupconné encore: elles ont leur action, leur repos, leur sommeil, leur veille, leurs habitudes (car ce n'est pas tout de suite, ce n'est qu'au bout d'un certain temps qu'elles perdent leurs heures ordinaires pour en prendre d'autres) et lorsque Delille, s'empressant de celebrer ces resultats en beaux vers, va jusqu'à dire:

"De la credule fleur le calice est trompé."

Ce language metaphorique de la poesie ne parait presque plus metaphorique.

Nevertheless he still leaves undecided, what the description of rays may be which acts most favourably in promoting vegetation, whether, for instance, the chemical or the luminous portion of the spectrum is most efficient; neither has he adduced from De Saussure a sufficient amount of evidence to overpower the weight due to the experiments of Mr Ellis, which tended to throw doubts upon the extent of the purifying influence attributable to plants.

I am, therefore, induced to flatter myself, that the researches[†] in which I have myself been subsequently engaged, with a view to the more satisfactory elucidation of these two points, would not have been regarded by this great botanist as altogether superfluous.

Decandolle has also presented us with a very ingenious theory with respect to the autumnal coloration of leaves and fruits, which he regards as due to an action exerted upon their colouring matter, by an acid generated within their tissue, when the latter has begun to lose, with its vitality, the power of separating carbon from oxygen.[‡]

He has also laid down a curious law with respect to the series of tints, which the flowers in each species may be brought to assume, founded upon the same chemical principles, and confirmed by observing the degree in which art is capable of modifying their natural colour.

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[*] See Liebig's remarks, in his Report "On Chemistry in its application to Agriculture and Physiology," 2d English edition, p. 30.

[†] See my Paper in the Philosophical Transactions for 1836, "On the action of Light upon Plants, and of Plants upon the Atmosphere."

[‡] The new photographic researches of Sir John Herschel on vegetable colours promise to lead to a better understanding of this subject. He finds leaves to contain two colours, the green destructible by light, the red not. Hence, when the activity of the vegetation no longer recruits the green principle, in the same ratio in which it is destroyed by the solar influence, the red predominates, and the sere or withered appearance of the leaf supervenes.—*See Philosophical Magazine for February* 1843.

Both these theories were suggested in the Flore Française published in 1805; and whilst the former has since been confirmed by the researches of Macaire, respecting the chromule of the leaf, the latter has been more fully developed by Schubler and Funk in a memoir published by them at Tubingen in 1825.

One of the ablest chapters in the work, may perhaps be considered the one in which he discusses the cause of the directions which the parts of plants severally affect.

He has here proceeded upon the sound, but too often neglected, principle, of declining to call in the aid of the vital principle, for the purpose of explaining phenomena which may be referred to physical causes alone. The maxim, " Nec Deus intersit," holds good in the natural sciences not less than in poetry.

Thus, instead of vaguely attributing the tendency of the stem to mount upwards, and that of the root to descend, to any principle so near akin to instinct or volition, as that of a disposition in the one to seek, and the other to avoid light, as some former physiologists had done, Decandolle shews, that both these phenomena would arise out of the difference in the mode of growth belonging to these two parts, and out of that in the way which certain external agents affect their organization.

Nor has our author been less successful, in applying those principles of vegetable physiology, which he had previously laid down, to the explanation of the differences which subsist between the various kinds of parasitical plants, and in establishing a classification of them founded on the above considerations.

He has pointed out that those parasites which insert their roots into the woody matter of the plant which nourishes them, require leaves, because they obtain the sap unelaborated; whilst those which are rooted merely into the bark, are destitute of leaves, because they draw their supply from the descending sap, which has already undergone the necessary preparation in the parent tree.

The former, such as the misletoe, may live on several different sorts of trees, because its own leaves enable it to bring about the requisite changes in the juice which it imbibes; but the latter are generally confined to plants of the same genus, or at most of the same natural family, since they possess no organs adapted for modifying the quality of the sap, so as to suit it to their own purposes.

Another question upon which new light appears to have been thrown by the writings of M. Decandolle, although many probably are not aware of the source from whence their present notions on this subject have been derived, relates to the indefinite duration of the life of a tree, as contrasted with the definite term of existence which nature has prescribed to every animal.

The former is destroyed merely by the accidents and diseases contingent upon the peculiar circumstances under which it is placed; the latter, if it were not carried off prematurely by the maladies to which it is exposed, would nevertheless at length perish from old age alone.

This distinction between a plant and an animal, which at present may appear to follow as a natural consequence from the fact, that, agreeably to the views of modern botanists, a plant is to be regarded, not as an individual, but as an aggregate of individuals, each bud being a new being, grafted upon the branch from which it issues, was warmly disputed when Decandolle first alluded to it in 1805.

Its universal admission at the present time affords a satisfactory proof, that the philosopher who sets out with sound principles, and is capable of deducing correct inferences from them, may often live to see those very opinions, which, when he first broached them, appeared to others the most paradoxical, take such deep root in the public mind, that their parentage shall be forgotten by the majority of those who adopt them as their own.

I next proceed to notice two points discussed in these volumes, upon which the conclusions arrived at by our author do not meet with such general concurrence amongst physiologists.

I will allude, in the first place, to his opinions with respect to the descending sap, on which subject he adopts the views of *the older botanists*, and maintains that nourishment, in a liquid form, flows down from the summit of every exogenous tree to its base, and that the new layers of bark and wood produced each year are formed by the pre-existing ones, and nourished by the juice which descends,

If these conclusions can be maintained, they may seem to bear him out in dissenting from the theory of M. Petit Thouars with respect to the growth of exogenous trees; but I must admit that they cannot be allowed to set aside the direct evidence which the latter botanist has brought together to prove, that fibres actually descend from the branches in certain endogenous trees, and penetrate between the rind and the old wood.

Are we, then, to suppose the mode of growth in these two classes of plants to proceed upon a different principle? or may we reconcile the two apparently conflicting statements, by supposing fibres to be sent down from the buds, and to constitute the rudiments of the young wood and of the young bark, admitting at the same time that these fibres require the descending sap to supply them with materials for further growth and development?

Without, indeed, pretending to gainsay the arguments alleged by Decandolle on the contrary side, I must freely admit, that the more recent observations of Mohl, with respect to the structure of endogenous stems, have afforded some additional, though indirect, confirmation of M. du Petit Thouars' theory, as to the fact that fibres descending from buds, may constitute, in endogenous trees, the woody matter of the stem.*

The reasons on both sides of the question are ably placed before us in Dr Lindley's Introduction to Botany; and a comparison of what is there said with Decandolle's remarks in his Physiologie Vegetale may be recommended to those whose disposition leads them to institute experiments on Vegetable Physiology, as being well calculated to set before them the doubts which still hang over the subject, and the true points which remain open for investigation.

Another portion of the volume, from which many may be inclined to dissent, is that which relates to the excretory function ascribed to the roots, from which our author has deduced an explanation of the deterioration which a plant is subject to, when sown year after year upon the same ground, as well as of the consequent advantages of a due rotation of crops in agriculture.

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* See also the Researches of Gaudichaud, in confirmation of M. Petit Thouars' theory. Paris, 1841.

All, I apprehend, will be disposed to allow his theory on this subject to be ingenious and plausible, and to admit that he has evinced considerable dexterity and skill in removing the objections that lie upon the face of such an hypothesis, and in particular in reconciling the permanent existence of forest trees on a particular locality, with the assumed necessity for a frequent change in the soil from which they draw their nourishment.

I conceive also, that subsequent observations have tended to confirm, rather than to invalidate, his fundamental position, that the roots are excretory, as well as secretory organs ;* and the experiments of Macaire and others seem to place beyond doubt the fact, that in plants which contain poisonous juices, the excretions are also poisonous.

It is likewise very probable, that the juices emitted by the roots of a plant, may be better adapted for the nourishment of certain species than of others ; and that hence each cultivated crop, and perhaps each kind of forest tree, may foster its own class of weeds, as well as its own particular parasites.

But it yet remains to be proved, whether the advantage of a frequent interchange of crops be connected in any degree with the emission of juices, which are more injurious to the plants of the same kind, or of similar conformation, than they are to others ; or whether, on the contrary, a different set of causes may not serve to account for the result.

The doubts, which I myself entertained at the time the theory was first propounded, have led me to originate a series of experiments, which may, I hope, tend eventually to throw some light upon the question ; and I must confess that these doubts have since been much increased by the results to which the latter have already led, no less than by the researches of Braconnot and others, which have lately been instituted with reference to the same question.

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* Thus, for instance, in Mr Hyett's experiments on the absorption of liquid solutions by growing timber, which are detailed in vol. xiv. p. 535 of the Transactions of the Highland Society, the neighbouring trees were found by him, as I understand, to have been affected by the poisonous matters imbibed, under circumstances, which seem to imply, that they had been excreted *by the roots* of the plants to the trunks of which they were applied.

I must not conclude my remarks on these two treatises, which, together taken, present us more completely than any other work of the kind, a statement of what is known respecting the structure and functions of the vegetable kingdom, without noticing those most useful appendices to either part, in which our author has followed up his delineation of the *explored regions* of botany, with a series of acute and searching questions calculated to point out to us also the *terra incognitæ* of the science.

A few of these may, indeed, have since been answered, but the greater number remain still unresolved; and I know not where the young experimentalist can better go, in order to learn in what direction his investigations may most profitably be carried on, or to what authority the student may more fitly appeal, in order to estimate the relative degrees of confidence that deserve to be placed on the statements of physiological writers, than to the questions alluded to, in which, as our author informs us, he has laid open to the world all the doubts, all the suspicions, all the schemes of research, which he had harboured in his mind, originally with a view merely to his own guidance and instruction in the scientific labours which he had proposed to undertake, but which he was now compelled, from the pressure of other engagements, to abandon to younger men.

It was previously to the publication of this work, namely, in the year 1830, that I had the advantage, during a residence of many months at Geneva, of hearing the principal points of theory which are therein embodied set forth in the course of lectures which their author annually delivered before the academy; and although his fellow-citizen, who has written a brief sketch of his life and writings in the "Federal newspaper," is mistaken in attributing my subsequent success in gaining the professorship of botany in this university to the certificate of capacity for that office which the professor was so obliging as to send to me when the vacancy occurred, as, *in point of fact*, the election had taken place before its arrival;

yet, I must freely own, that but for my stay at Geneva, I might never have obtained a sufficient mastery of the subject, to have held with any satisfaction to myself the Office I have the honour to discharge in this Seat of Learning.

Unacquainted as I was, until the period of my residence at Geneva, with any other than the artificial method of classification, it required the influence of such lectures, and of such writings as those of Decandolle, to remove from my mind the prejudices arising from early association, and to invest the science of botany with dignity and importance in my eyes.

And though I have since attended, with much satisfaction, the course delivered in London by Professor Lindley, and have corrected on many points the notions I received from Decandolle by the subsequent perusal of the papers of Mirbel, Brown, Schleiden, and others; yet it is but fair to admit, that it was at Geneva I first began to estimate at their true weight the pretensions of botany to be regarded as a science, and not merely as an ingenious art for discovering the name of any plant that might be put before us.

I then began to comprehend on what principle a certain acquaintance with botany was inculcated at the academy at Geneva as constituting an essential part of a liberal education, perceiving that, like every other branch of natural knowledge, if prosecuted in a philosophical spirit, and with a constant reference to first principles, it might be capable of serving an important purpose in training and disciplining the mind of the student.

For the furtherance indeed of this object, the lectures of few Professors could have been better adapted than those of the individual who forms the subject of my memoir, combining, as he did, great powers of generalization, with a most extensive acquaintance with facts relating to all branches of natural history, and able, from his correct and classical taste, as well as from his perfect clearness of understanding, to render the most technical portions of the subject interesting, and the obscurest intelligible.

For my own part, I can only say, that, although by no means familiar with the language in which he spoke, I followed, nevertheless, the thread of his discourse, even when it related to the more intricate points of structural or physiological botany, with as much ease, and with as thorough a comprehension of its drift and import, as I ever recollect doing that of the professors whose instructions conveyed to me the greatest amount of information in my own country.

I recollect Monsieur Decandolle, with what might be considered a pardonable piece of vanity in such a case, relating to me an anecdote, which illustrated, in a very striking manner, the estimation in which his style and language was held by better judges than a foreigner like myself.

Amongst his hearers, on one occasion, was the celebrated De Bonstetten, the friend of Madame de Staël, at that time nearly 80, who, though he had evinced at former periods of his life some interest in physical phenomena, was, nevertheless, chiefly distinguished as a man of letters. M. Decandolle observed him occasionally taking notes at his lectures with much diligence, but this was not at the parts which appeared most worth remembering for the purposes of a botanical student.

His curiosity was therefore excited, and he was tempted one day, after the lecture was over, to ask Monsieur de Bonstetten on what principle he proceeded in selecting passages for particular annotation. " Oh, don't flatter yourself," said his friend in reply, " that I am come here, at my time of life, to learn botany ; what I am curious about is to observe, how, when you get involved in the mazes of a difficult sentence, you manage to disentangle the thread of your discourse, and to round off your period."

But it was not merely the excellence and beauty of Monsieur Decandolle's lectures that rendered Geneva, at the time I speak of, an admirable school for acquiring botanical knowledge.

His extensive herbarium and library were at all times open to the student, as well as to the more advanced cultivator of this science ; and I can well recollect, that the rooms which contained it were the daily rendezvous of a number of individuals, Genevese as well as Foreigners, whose names are not unknown amongst the cultivators of Natural History.

Here, besides that accurate practical botanist Monsieur Seringe, who acted as curator to Decandolle's museum, and is now Professor of Botany at Lyons, I used to meet Professor

Choisy, who, though principally occupied on the Philosophy of the Human Mind, has nevertheless found time to publish several profound memoirs on botanical subjects; Vaucher, who still retained at this late period of his life an ardour for natural history; Moricand, the author of the Flora Venetiana and other memoirs; and, amongst foreigners, Blith, now Professor at Upsala; Wydler, distinguished for researches on the Impregnation of Plants; Dr Macreight, now resident in Jersey, who has since published an excellent Synopsis of British Plants; and others, whom I have not time to enumerate.

His influence in the city of Geneva was proportionate to the zeal he displayed, and to the talents he put forth, in the advancement and diffusion of botanical science. To him we owe the foundation of the Botanical Garden at Geneva, as well as of its Museum of Natural History, two institutions which were created, mainly, at least, by his influence and exertions;* and in botany, so lively and so universal was the interest he inspired, that when, on a certain occasion, he was called upon unexpectedly to return a collection of designs of Mexican plants, of which he had obtained the loan, one hundred ladies of the place came forward of their own accord, and in ten days completed for him copies of no less than 1000 of these drawings, to take the place of the originals which he was about to surrender.

It was in the autumn of the year 1840 that I saw, for the last time, at Geneva, the subject of this memoir. He was then just returned from the Scientific Congress at Turin, where he had been received with all the honours due to his distinguished talents, and to his long services in the cause of natural history.

He appeared to me much broken in constitution, but with his mental faculties unimpaired; and although he spoke, as might be expected, with some degree of despondency, as to the prospects of his completing the Prodromus, the great object of his ambition, he nevertheless dwelt with satisfaction on having brought to a close that most formidable portion of it which related to the immense family of the Compositæ, and on the assistance he should derive in the subsequent parts from the monographs already drawn up of several of the more important natural families which would come before him, as from that of the Labiatæ by Mr Bentham.

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* He shewed much address in extracting subscriptions from his fellow-citizens for the furtherance of these scientific objects. One of his friends he would persuade to pay for the wood-work of a cabinet, another for the glass, whilst a third would be prevailed upon to stock it with specimens.

His death, which took place in September of the following year, has indeed frustrated these hopes and aspirations; but the public may doubtless look to the completion of this most useful undertaking under the auspices of the same Name which figures at its commencement; as his son, who has succeeded him in his professorship at Geneva, and who is already distinguished by his Memoir on the Campanulaceæ, and by other writings, will, I trust, bring to a successful termination a work in which the reputation of his illustrious parent is so deeply involved.

This, however, is a point, upon which it does not become one who, like myself, have, since the year 1830, known Monsieur Decandolle only through his writings, to affect to speak with authority; and, for the same reason, I shall forbear to dwell upon his private worth, or on his public services, as member of the Legislative Body, and as Rector of the Academy of Geneva, confident that justice will be hereafter done to these his deserts, by persons who had more opportunities fully to appreciate such points in his character.

Observations on Subterranean Temperature in the Mines of Cornwall and Devon. By W. J. Henwood, C.E., F.R.S., F.G.S., &c. &c. &c.*

My geological survey of the mines in the Duchy of Cornwall afforded many opportunities for observations on subterranean temperature, of which I always availed myself.

After most careful consideration of the subject, and consultation with others who have also been engaged in this inquiry, it has been thought best to confine the observations, as much as possible, to the temperature of the streams of water immediately issuing from the unbroken portions of the rocks and veins.

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* From the Transactions of the Royal Geological Society of Cornwall, vol. v., at present in the press.

The reasons for this preference are:—That the temperature of the *air* in mines is affected, not only by the presence of the workmen, the combustion of candles, and the explosion of gunpowder, but also by the warm or cold air which is brought to the same spot, by the varying directions of the currents underground, which are more or less influenced by the changes of wind at the surface;—that the *rocks*, forming the sides of the shafts and levels, must, to a certain extent, partake of the temperature of the air circulating through them, and, of course, be affected by its changes;*—and, that the water flowing through, or standing in pools, in the levels, is exposed to the same modifying causes, and probably, also, warmed by the workmen who frequently stand in it.

As all these causes operate irregularly, it is difficult, if not altogether impossible, to select any distant periods at which their influence should be the same. The present observations were therefore made on the water as it issued from the unbroken rock, before the streams could be affected by the temperature of the levels; and they were, for the most part, instituted where frequent excavations scarcely permit even the apertures to partake of that influence, whilst the more deeply seated portions of their channels are perfectly free from it.

The following table presents a comparison of the temperatures prevailing at nearly similar depths, in the different districts in which the mines of Cornwall and Devon seem naturally grouped:—

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* "During his observations on the hourly variations of the magnet, Reich had an opportunity of observing the rapidity with which the air operates upon the rock even at a distance of 40 inches. The air in the mine shewed 48°.6 with but slight variations, and a thermometer sunk into the rock 48°.64; but when, after 44 hours' observation of the magnet, the temperature of the air had been raised by the presence of the observers, and their two candles, to 49°.7, the thermometer in the rock, which was subject to no change of air whatsoever, was found to have risen to 48°.71, 48°.73. This destroyed all hope of obtaining the temperature of the rock, quite free from the influence of the air, by sinking thermometers even 40 inches deep into the rock."

TABLE I.

DISTRICTS.	Surface to 50 fathoms.		50 to 100 fathoms.		100 to 150 fathoms.		150 to 200 fathoms.		200 fathoms and beyond.		Means.	
	Depth fathoms.	Temp.	Depth fathoms.	Temp.	Depth fathoms.	Temp.	Depth fathoms.	Temp.	Depth fathoms.	Temp.	Depth fathoms.	Temp.
Saint Just . . .	25·	51°·45	70·	56°·17	128·	62°·75	156·	61°·0*	—	—	95·	57°·84
Saint Ives . . .	28·	54°·16	73·	59°·64	119·	67°·0	197·	65°·0*	228·	72°·0*	129·	63°·56
Marazion	33·	58°·62	64·	61°·66	131·	71°·33	—	—	—	—	76·	63°·87
Gwinear, &c. .	28·	57°·1	78·	63°·53	131·	66°·0	167·	76°.0	—	—	101·	63°·4
Helston	31·	56°·6	80·	62°·1	133·	66°·1	190·	68°·5	237·	80°·0*	134·	66°·66
Camborne, &c.	21·	53°·3	78·	61°·5	125·	66°·54	168·	67°·17	—	—	98·	62°·13
Redruth, &c. .	33·	52°·7	76·	58°·3	130·	70°·64	177·	86°·07	245·	89°·17	132·	71°·37
Saint Agnes . .	39·	58°·5	75·	63°·9	129·	69°·0	154·	72°·25	—	—	99·	65°·91
Saint Austell .	—	—	52·	55°·0*	111·	71°·0	160·	67°·75	220·	88°·75*	136·	70°·62
Tavistock, &c.	30·	54°·3	67·	58°·75	118·	64°·16	—	—	—	—	72·	59°·07
Means	30·	54°·87	72·	60°·88	127·	67°·43	173·	78°·0	240·	85°·52	112·	66°·88

* From single observations only.

TABLE II.

Mean Temperature at nearly equal depths in the granite and slate rocks.

DEPTHS.	Granite.		Slate.	
	Depth fathoms.	Temp.	Depth fathoms.	Temp.
Surface to 50 fathoms	25·	52°·67	30·	55°·9
50 to 100	71·	57°·68	73·	61°·9
100 ... 150	132·	65°·0	125·	68°·14
150 ... 200	161·	65°·7*	174·	79°·17
200 fathoms and beyond........................	240·	76°·15	241·	89°·4
Means...............	94·	60°·35	116·	68°·89

* From five observations only.

TABLE III.

Mean Temperatures of the rocks, cross-veins, and lodes, at nearly equal depths.

DEPTHS.	Rocks.		*Cross-veins.*		*Lodes.*	
	Depth fms.	Temp.	Depth. fms.	Temp.	Depth fms.	Temp.
Surface to 50 fathoms, ...	30·	55°·52	31·	53°·76	29·	54°·83
50 to 100	70·	60°·2	76·	61°·2	71·	59°·87
100 ... 150	134·	69°·66	115·	64°·75*	126·	66°·88
150 ... 200	180·	82°·11	163·	74°·4	161·	72°·41
200 fathoms and beyond,	235·	87°·9	220·	88°·75*	246·	88°·57
Means,...............	111·	67°·55	99·	64°·82	111·	66°·04

* From only two observations.

TABLE IV.

Mean Temperatures, at nearly equal depths, in the lodes which contain ores of different metals.

DEPTHS.	Tin *lodes.*		*Lodes* yielding both tin & copper ores.		Copper *lodes.*	
	Depth fms.	Temp.	Depth fms.	Temp.	Depth fms.	Temp.
Surface to 50 fathoms, ...	27·	53°·14	33·	55°·06	32·	56°·9
50 to 100	71·	59°·15	72·	61°·16	74·	61°·8
100 ... 150	129·	65°·92	124·	66°·09	127·	68°·39
150 ... 200	181·	64°·81*	171·	81°·75†	172·	78°·33
200 fathoms and beyond,	230·	74°·3	—	—	244·	89°·14
Means,...............	92·	60°·69	74·	61°·45	140·	72°·39

* From three observations only. † From four observations.

The following table (viz. Table V.) indicates the respective ratios of increase in temperature, expressed in fathoms of descent requisite to produce an elevation of one degree. The columns 2 and 3 are formed from Table II.; 4, 5, and 6, from Table III.; 7, 8, and 9, from Table IV.; and the last is the arithmetical mean of all the others. The eight columns are therefore deduced from the same facts, grouped in three different ways; it may therefore be anticipated that there will be considerable resemblance—between some of them, at least.

The temperature of the air at Plymouth and Penzance, which are near the eastern and western limits of the mining districts of Devon and Cornwall, has been very accurately* determined; and the observations made at several intermediate spots† may, perhaps, be equally relied on.

But this temperature, though affecting the rock to the depth of several feet,[*] cannot extend its influence very far; and, consequently, the observations made upon it will not, perhaps, avail much in the present inquiry.

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* The mean temperature of Plymouth is stated, by Mr Harris, at 52°.081; Reports of the British Association, vii. p. 24: that of Penzance, at 52°.0, by Mr Giddy, Phil. Mag. and Annals, iii. p. 182.

† Observations on the Temperature of Truro are recorded in the Reports of the Royal Institution of Cornwall; and of Falmouth, in the Reports of the Royal Cornwall Polytechnic Society.

No experiments have been made in Cornwall to determine at what depths, in different soils and rocks, at various periods of the year, the effects of atmospheric temperature cease, and those of subterranean heat commence.

Mr R. W. Fox's observations[†] on the temperature at the depth of a few feet, are the only ones here with which I am acquainted; and, because in many of the wells and shallower parts of the mines which I have examined, I have obtained the same average, viz. about 50 degrees, I concur with him[†] in thinking that we may begin our computations at that temperature; and I have, accordingly, taken it as my point of departure.

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[*] Professor Forbes, from observations made in the neighbourhood of Edinburgh, states, on the average of three years' experiment, that the effects of atmospheric temperature will become insensible at the following depths, in different substances :—

Trap-rock,............	55.5 feet.
Sand, ...	65·8 ...
Sandstone,............	96.1 ....

Reports of the Brit. Assoc. (1840), p. 435.

At Paris the effects of atmospheric temperature ceased at 25 feet; whilst in various Prussian mining establishments it was found to extend to depths varying from 27 to 63 feet.

Professor Bischof, *Edin. New Phil. Jour.* xxiii. (1837), p. 341.

[†] The bulbs of the thermometers were sunk to a depth of three feet below the surface, and the mean annual temperatures observed were,—

At Wheal Gorland (granite).........	48°.09
... Dolcoath (slate)...............	49°.94
... Falmouth (slate).....................	50°.67
Mean,............	49°·86

Mr R. W. Fox, *Corn. Geo. Trans.*, iii. p. 326.

Corn. Geo. Trans., iii. p. 326, and Reports of Brit. As. (1840), p. 310.

TABLE V.

DEPTHS.	Granite.	Slate.	Rocks.	*Cross-veins.*	*Lodes.*	Tin *lodes.*	*Lodes* yielding both tin & copper ore.	Copper *lodes.*	Means.
	Fms.	Fms.	Fms.	Fms.	Fms.	Fms.	Fms.	Fms.	Fms.
Surface to 50 fms.	9·3	5·0	5·8	8·2	6·0	8·6	6·5	4·6	6·8
50 to 100 fms.	9·1	7·1	8·1	6·0	8·3	7·3	6·4	8·5	7·6
100 ... 150 ...	8·3	8·3	6·7	11·0†	7·8	8·5	10·5	8·0	8·7
150 ... 200 ...	—*	4·4	3·7	4·9	6·3	—‡	3·0§	4·5	4·5
200 fms. and beyond, ...	7·5	6·5	9·5	3·9†	5·2	5·1	—	6·6	6·4
Means,......	8·5	6·2	6·7	6·8	6·7	7·3	6·6	6·4	6·8

* Five observations only.
† From two observations only.
‡ From only three observations.
§ From four observations.

(1.) As the mining districts vary considerably in their geological characters, it might have been expected that the temperatures at equal depths would not exactly coincide in all. Of this inequality, which is very conspicuous in Table I., it is not easy, and perhaps not possible, to give a satisfactory explanation. As the situations of the mines differ very much in their elevations, in very few instances do equal depths below the surface hold the same positions with regard to the sea-level. This is probably one cause of the observed irregularities in temperature; but it will presently be seen that there are others, which depend on the geological characters of the rocks and veins.

Table I. shews that the subterranean isothermal lines are not exactly parallel to the configuration of the surface;* although, in many instances, there is a sort of distant resemblance in their outline.

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* "A chthonisothermal line of any temperature, which was under a small district parallel to the surface, cannot continue its course under a neighbouring mountain, either parallel to the external configuration of the mountain, or in the continuation of its former direction, but must curve upwards." Professor Bischof, Jameson's *Edin. New Phil. Journ.*, xxiv. (1838), p. 146.

A detailed table, which I have prepared, presents many examples of different temperatures at equal depths; not only in the same mines, but even in various parts of the streams of water flowing out of the very same crevices and apertures, within a few feet, or even inches, of each other. It need, therefore, excite no surprise that there is not a perfect uniformity in the temperatures of tracts miles distant from each other.

(2). No attempt to determine the difference between the mean temperature of the granite and slate, at various depths, was made before this inquiry had been considerably advanced;* although the fact had been alluded to,† and had been, from time immemorial, known to practical miners.‡

This difference is exhibited in Table II., and is far more conspicuous in the deeper than in the shallower levels.

The granitic rocks may be more exposed to the cooling influences of descending streams, as their structure permits the percolation of water more readily than that of the slate-series.

(3). The general impression has been, that the temperature of the rocks is lower than that of the veins:§ the results of my inquiries, however, conclusively demonstrate, that, at all depths, the rocks are warmer than the *lodes*, and the *lodes* than the *cross-veins*. (Table III.)

It is well known to miners that the largest streams of water flow through the *cross-veins*;‖ smaller ones through the *lodes*;

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* My own Papers, Thomson's Records of General Science, iv. (1836), p. 198; Reports of the British Association, vi. (1837), p. 36.

† Mr R. W. Fox, Annals of Philosophy, iv. (1822), p. 447; Phil. Mag. and Annals, ix. (1831), p. 98.

‡ "From observations carried on in various mining establishments in the Prussian dominions, M. Von Dechen finds that the increase of temperature is, in general, much more rapid in coal than in metalliferous mines." Professor Bischof, *Edin. New Phil. Journ.*, xxiv. (1838), p. 141.

§ Mr R. W. Fox, Corn. Geo. Trans., ii. p. 21; Report of the Royal Corn. Polytech. Soc. (1836), p. 107; Dr Forbes, Corn. Geo. Trans., ii. p. 217; my own Summary of Experiments made in Cornwall (by other observers), Edin. Jour. of Science, x. o. s. (1829), p. 244.

‖ As a general fact, the *cross-veins* traverse the *lodes*; and thus, by falling in contact with and separating all of them, become the main subterranean aqueducts or channels for the circulation of water.

whilst but little issues from the rocks, whether granitic or slaty. It is an equally recognised fact, that a considerable portion of the water pumped out of our mines has been rain-water, which must have entered the ground at a temperature nearly the same as that of the atmosphere; consequently, much lower than that prevailing at even comparatively small depths in the mines.

It therefore is obvious, that, as the *cross-veins* receive the largest quantity of this cold water, they will be most affected by its cooling influence;[*] whilst, as the rocks absorb the smallest proportion of it, they will therefore suffer the least depression of temperature.

That this is one cause of the observed difference can admit of no doubt; whether it is the only one, is foreign to this inquiry.

It may be true that the ascent of vapour, which would tend to raise their temperature,[†] is facilitated by the more porous

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[*] At 264 fathoms deep, in Mr Pemberton's Colliery, at Monk-Wearmouth, Professor Phillips found, that, as bubbles of gas rose through the water, its temperature fluctuated; in one case, from 69°·1 to 69°·7, and in another, from 71°·6 to 72°·6. Lond. and Edin. Phil. Mag., v. (1834), p. 449.

At 230 fathoms deep, in *Dolcoath*, Mr R. W. Fox placed a long thermometer in the copper *lode;* and, unless overflowed by water, it for many months indicated a temperature varying only from 75°·0 to 75°·5. Cornwall Geo. Trans., ii. p. 27.

But in metalliferous districts, the constancy of the temperature of water issuing from the same spot, at distant periods, must depend on the subterranean works remaining unaltered; for, if the deeper levels are untouched, and the shallower ones are extended, the warm water from below will rise as before, whilst the cold from above will be intercepted, and thus the temperature of that flowing from intermediate spots will rise. If, on the other hand, the shafts are deepened and levels *driven* beneath, whilst the shallower ones are unwrought, the former will intercept the warm water and prevent its rising, and the cold water from above still flowing as before, the temperature of intermediate stations will decline.

Such, in fact, is the case at *East Wheal Crofty*, where the temperatures at two different spots, within two years, diminished—the one 3°·0 to 7°·25, and the other 7°·0,—both parts of the mine having in the interval been deepened 30 fathoms.

Whatever cause may increase the water from above, must lower the temperature; whilst any addition to that from below must elevate it.

[†] Mr R. W. Fox, Corn. Geo. Trans., ii. p. 16.

nature of the veins; but, on the other hand, it must not be forgotten, that this porous structure also affords a readier passage for the descending water.

(4). The working miners of Cornwall have long known that the *lodes* containing tin-ores are, at equal depths, colder than those in which copper-ores occur. This has been noticed by Mr M. P. Moyle;* but no attempt has, until now, been made to determine the exact difference between them.

Table IV. clearly proves the truth of this popular opinion; and indicates that the *tin-lodes* possess the lowest temperature,† and *copper-lodes* the highest; whilst the *lodes* in which the ores of both metals are mixed hold, in this respect, an intermediate position.

(5). The general fact of a progressive elevation of temperature as we penetrate farther into the crust of the earth, naturally leads us to inquire the ratio of its increase.

The manner in which Table V. is constructed has been already described; and as the ratios presented by the different varieties of rocks and veins are there exhibited connectedly and at one view, it is needless to dilate on them.

From the surface to 150 fathoms deep, the rise of temperature, for equal increments of depth, seems to be in a diminishing ratio,—a fact previously known.‡ But deeper observations disclose the curious, and, as it would seem, almost anomalous circumstance, that at more than 150 fathoms deep, the progression again becomes more rapid; and that the ratio at about 150 fathoms in depth is at a minimum, and increases, both at greater and smaller depths.

Whether further experiments may confirm or disprove the generality of this fact, I do not pretend even to conjecture;

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* Mr R. W. Fox, Corn. Geo. Trans., ii. p. 414.

† The same general fact is also known in Germany; for Professor Bischof says,—"The tin-mines of Sauberg at Ehrenfriedersdorf shew a remarkably low temperature: indeed, it is a prevailing opinion there, that stanniferous mountains are colder than others. . . . The low temperature of Heinrichssohle, in the Altenberg district in the Erzgebirge, was also ascribable to the rocks of tin-stone." *Edin. New Phil. Journal*, xxiv. (1838), p. 140.

‡ Mr R. W. Fox, Reports of the British Association (1840), p. 315.

but I must remark, that the number of observations I have made at more than 150 fathoms deep is very considerable.

The various ramifications of the great adit in the Gwennap mining district, have an aggregate extent of between thirty and forty miles. It drains a tract of about 5550 acres in area, and discharges nearly 1500 cubic feet of water per minute. Rather less than one-third of this stream is collected at the adit level, whilst the remainder is pumped up from a mean depth of about 190 fathoms. Its temperature varies between 60°·5 and 68°·0, and is, on an average, more than 12° above the mean of the climate.

The subject of subterranean temperature attracted attention in France and Germany long before it did so in this country.* Though I have made some progress in a comparison of the results there observed with those obtained here (and such an inquiry would embrace many points of interest), I must defer its completion until a more convenient opportunity; as, in this communication, I have purposely confined myself to my own observations in Devon and Cornwall.

4 Clarence Street, Penzance,
16*th January* 1843.

Summary of Results on the Fossil Animals of the Chalk Formation, still found in a living state. By Professor Ehrenberg of Berlin.†

It should be the endeavour of one who has collected new facts not merely to bring them accurately and clearly under view, and to compare them according to his own idea of them with the existing state of science, but also to elucidate the conclusions which directly and necessarily result from them. This additional task is generally very difficult, sometimes leading to the discovery that what had been supposed new was not so, or not of sufficient importance for such an extended investigation; sometimes rendering a fresh and more profound examination requisite, or misleading to an evident exaggeration of the facts discovered, and to conclusions which they do not justify, and is therefore very frequently avoided from motives of fear or convenience. To leave this to others lightens indeed the labour of the task; but this alleviation, at the same time, lessens the value of isolated observations, and throws a doubt upon the care employed in making the comparison.

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* M. Daubuisson, des Mines de Freiberg, i. p. 258; ibid. iii. pp. 151–186–200; M. de Trebra, Annales des Mines, i. (1816), p. 377; ibid. iii. (1818, observations made 1805–7), p. 59; M. Hassenfratz, ibid. i. p. 378; M. Le Baron Cordier, Sur la Température de l'intérieur de la Terre, Memoires de l'Académie des Sciences, vii. (1827); M. J. Levallois, Annales des Mines, iii. (1833), p. 629; M. Reich, Beobachtungen ueber die Temperatur des Gesteins in verschiedenen Tiefen in den Gruben des Sachsischen Erzgebirges in den Jahren 1830 bis 1832; Prof. Bischof, Edin. New Phil. Journal, xxiii. (1837), p. 330; ibid. xxiv. (1838), pp. 132–252.

† Extracted from page 160, 161, 162, 163, 164, of a memoir in the Berlin Memoirs for 1839; but read before the Academy on the 17th and 31st of October, and 16th February 1840. Translated for and inserted in No. XI. of Taylor's Scientific Memoirs.

Along with the general view of the facts advanced, I have also aimed at forming comparisons and conclusions, not in order to veil any erroneous view of the facts, but to render it the more conspicuous where it had gained ground; and, on the other hand, to render the truth discovered more striking, and thus to awaken a more general and active interest for this kind of inquiry, I shall only draw such conclusions as are most obvious, since the further we depart from actual observation, the more we deviate into the field of uncertain speculation, which, when constructive, instead of being completive nearly, becomes the very opposite of philosophical inquiry, and is just as feasible for anybody as for the philosopher himself. I would desire, that my conclusions should always be less rather than more than the observations might warrant me to draw.

1. There are numerous animals of the chalk formation which are still found living, and precisely such as do not, either from great variation of form within generic limits, or from the simplicity of their exterior, leave any uncertainty in determining their specific difference.

2. Of the animal forms which constitute the greater mass of the white chalk, those which preponderate in number of individuals are identical with living species; and hitherto all the principal species which form the rocks, have been observed

alive even in the short time during which the inquiry has been proceeding.

3. The principal number of species, and the great mass of individuals of these recent forms, are microscopic infusoria with *siliceous shells*, and Polythalamia with *calcareous shells*, scarcely or not at all perceptible to the naked eye, which nevertheless form so incalculably great a volume of the solid portion of the earth, that the few species asserted to be still living, from other groups of animals of higher organization, even if they were all decidedly identical, bear not the slightest comparison with the number and mass.

4. The microscopic organisms are, it is true, far inferior in individual energy to lions and elephants; but in their united influences they appear far more important than all those animals.

5. The fifty-seven recent species of the chalk in Europe, Africa, and Asia, do not live solely or principally in southern latitudes, as has been shewn with respect to the recent larger forms of the so-called Eocene formation, but have been observed living both in those and in northern latitudes. These recent species are not rare nor isolated, but fill, in incalculable numbers, the seas of northern Europe, and are not wanting on the tropical coasts of the American ocean.

6. The idea that the temperature and constitution of the atmosphere and oceans were essentially different at the period of the chalk formation, and adverse to the organized beings at present existing, naturally acquired more probability and weight, the more decidedly different all the creatures of that period were from those of the present time; but loses more and more in importance the less the chalk proves to be a chemical precipitate, and the more numerous the forms agreeing with those of the present day become by renewed inquiry. Nay, there is not the least doubt that the perfectly ascertained identity of a single species of the present day with one of those of the chalk, renders doubtful the necessary transformation of all the others subsequently to the formation of the chalk rocks; how much more so when these are numerous, and such as form masses! The size appears to be of no importance, as the small organisms have

already been shewn to agree with the large, with regard to the effect of external influences upon them.

7. The period of the dawn of the organic creation co-existent with ourselves, can only be admitted as being anterior to, and below, the chalk formation, if indeed, which is questionable, such a distinction can be made ; or the chalk, with its rocks, covering far and high the superficies of the earth, forms part of the series of recent formations, and some of the four as yet well established great geological periods of the earth's formation, the quaternary, tertiary, and secondary formations, contain recent organisms, it is, as three to one, more probable that the transition or primary formation is not differently circumstanced ; but that, from the gradual longer chemical decomposition and change of many of its organic relations, it is more difficult to examine and determine.

Paludina vivipara and *Cyclas cornea* of the Weald clay, and the recent *Trochus* below the chalk, according to Defrance, as well as the confirmation of the occurrence of *Terebratula caput Serpentis* in the upper Jura formation, by Von Buch, together with my observations of microscopic, yet, nevertheless, peculiar Polythalamia in the flints of the Jura, are additional positive indications of the inconceivable extent of similar organic relations, the further investigation of which is one of the important questions to be determined in the present age.

8. It cannot be denied that the notion hitherto frequently asserted, that all recent organisms, including man, are the descendants and perfected stages of metamorphoses of trilobites and ferns, has something in it opposed to sound sense ; when, therefore, the direct inquiry leads powerfully to a different point of view, it has much in its favour, even though it be reserved to a future period, to explain the vast connection of the phenomena.

9. Since now, Polythalamia, and other forms identical with chalk animals, exist, which are not endowed with spontaneous division, this faculty of the Infusoria, and their general nature, are not the sole causes to which the indefinite duration of the species is owing.

10. In consequence of the mass-building Infusoria and Polythalamia, the secondary formations can now no longer be distinguished from the tertiary; and in accordance with what has been above stated, masses of rock might be formed even at the present time in the ocean, and be raised by volcanic power above the surface, the great mass of which would, as to its constituents, perfectly resemble the chalk. Thus, then, the chalk remains still to be distinguished by its organic contents as a geological formation, but no longer as a species of rock.

11. The power so conspicuous in the organic beings under consideration is, according to experience, so immensely great, even in its influence on the inorganic, that, with the concurrence of favourable circumstances, they alone might give rise to the greatest changes in the distribution of the solid land of the earth in the shortest space of time, especially in the water; and the ascertainable extent of such influences, however great, remains constantly small in comparison to those that are possible, consequently do not give, by their magnitude, any certain measure of periods of time.

12. The correctness of the above expositions is not founded on individual opinion, formed from hasty inspections of trifling objects; but the microscopic objects on which the opinions are based (though fading from our notice as individuals, yet, by their number, forming mountains and countries), are accessible to any comparison in distinct preparations, made according to the methods already described; and almost all the forms here mentioned, especially all the more important ones, have been carefully preserved by me, and laid before the Academy.

13. *Thus then, there is a chain, which, though in the individual it be microscopic, yet in the mass a mighty one, connecting the organic life of distant ages of the earth, and proving that it is not always the smaller, or most deeply lying, which is the base and the type of those which are larger and nearer the surface on our earth; and, moreover, that the dawn of organic nature, co-existent uith us, reaches farther back into the history of the earth than had hitherto appeared.*

On a method of Registering the Force actually transmitted through a Driving-Belt. By EDWARD SANG, Esq., F.R.SS.A., Professor of Civil Engineering, College, Manchester. Communicated by the Royal Scottish Society of Arts.*

It is a desideratum to have the means of ascertaining how much force is actually consumed in the working of a machine. Whenever the motion is communicated by the intervention of a belt or band, this can be very easily accomplished.

When we see a belt passed over two pulleys, and look without any narrow examination at the motion, we regard the action as a very simple one; there is more in it, however, than appears at first sight. For the sake of clearness, let us call the driving-pulley the drum, and the other the pulley. The belt passed over them, whether plain or crossed, has two free parts, one of which *draws* and the other of which *follows*. If it were possible that no force were needed to turn the pulley, these two free parts would be in the same state of tension; but whenever any resistance is made to the motion of the pulley, the drawing part is distended more, and the following part less, than usual; and experiments show, that, within all practical limits, this change is exactly proportional to the pressure necessary for overcoming the resistance.

As the movement proceeds, the distended part of the belt is lapped over the drum, and, so to speak, the contracted part is lapped over the pulley, so that the circumference of the drum moves more swiftly than that of the pulley; thus, if the distension be 1 in 100, for 100 inches of the drum there would only be 99 inches of the pulley passed over.

The difference between the velocity of the drum and that of the pulley thus indicates the pressure needed to carry the drum round. Now, this pressure, combined with the distance through which it acts, gives the force used; and hence, the simple difference between the distances passed over by the circumference of the drum and by that of the pulley is exactly proportional to the force; and we have only to contrive some method of registering this difference, in order to have a record of the total force transmitted by the belt.

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* Read before the Royal Scottish Society of Arts 9th January 1843.

There may easily be contrived a variety of arrangements for shewing the difference between the motions of the drum and pulley. Thus a pair of indicators may be fitted, one to each shaft, so as to tell the total number of turns made by each; from this number, by help of the measured diameter, the distance passed over by each circumference can be found, and thus the element for knowing the force transmitted can be had.

Or otherwise, and this perhaps is the most convenient arrangement, a light pulley, having its circumference one foot, may be brought to bear against the belt on the drum, and another against the belt on the pulley; if these light pulleys have counting geer attached, a simple reading off and subtraction will give the difference of distance.

Having now ascertained the difference between the motions of the drum and pulley, it remains to ascertain by what this must be multiplied, in order to give the force. It is not my object, at present, to enter into the theory of the matter—although this theory presents several points of considerable interest—but to give a practical application of the principle. In order to find out the force due to a single foot of difference, we have to run the pulley unburdened for a considerable time, taking notice of the difference of motion, and then loading the shaft by means of a spring friction-strap with two arms, repeat the observation over as many strokes of the engine or turns of the drum; in this way we shall have a new difference, and subtracting the one from the other, we shall have what is due to the force as shewn by the friction-strap.

When the multiplier for one belt has been ascertained, that for any other belt may be approximately computed, if it be of the same material, by having regard to the relative weights of a foot of each; so that a pair of accurately constructed counters form a portable apparatus, by means of which the force transmitted by any belt may at once be ascertained, the weight, length, and material of that belt being known.

MANCHESTER, 1*st Sept.* 1842.

On the English Arc of the Meridian. By WILLIAM GALBRAITH, Esq., M.A., Vice-President of the Royal Scottish Society of Arts, F.R.A.S., &c.* Communicated by the Royal Scottish Society of Arts.

The paper which I now lay before this Society is one relative to a very important branch of science, whatever may be its own merits. The English arc of the meridian, betweeen Dunnose in the Isle of Wight, and Clifton on the southern borders of Yorkshire, extends through a tract of country of about 200 miles, and has created considerable discussion in foreign journals, as well as in those of this country. The notion of errors having been committed in the original zenith-sector observations to determine the length of the celestial arc corresponding to that measured on the earth's surface, is now generally exploded; since instances of much greater differences between the observed and geodetic latitudes have occurred in many extensive similar operations on the Continent.

M. Bessel has indeed discovered that there has been a very considerable error committed in the original reduction of the observations made on the star Capella, amounting to about 18″; but, as this error was similarly applied to the observations at both the north and south ends of the arc, it produced little effect on the intercepted arc,—the only result then deduced.

M. Bessel has, however, determined the latitude of Dunnose carefully from the zenith-sector observations, and, in this case, he could not avoid detecting this grave error, since it would have produced an equal effect on the observed latitude.

The original bases from which the sides of the triangles are determined, were not measured in the imperial standard, and therefore all the distances given in the Survey require to be reduced to it. M. Bessel has made this reduction correctly according to Kater's experiments; but he has *not* recomputed the triangulation. He took merely the final result as it stood in the survey. This part of the operation forms the subject of the present paper, and it is hoped that the results (not differing greatly from Bessel's) of my enquiries are stated in such moderate and candid language that no offence can, by any possibility, be reasonably given to any gentleman either formerly or now connected with this great national work, so highly beneficial to the interests, and, on the whole, creditable to the science of the country.

These operations have been, and still continue to be, of great service to practical science, as well as to the useful arts. In many instances, the Ordnance Survey in this country has been of great importance in the selection of lines of railways, canals, and roads. On the Continent, the railway from Paris to Lille, with the branches to Valenciennes, Dunkirk, Calais, and Boulogne, derived great advantages from the new Survey of France, both with regard to economy in time and diminution of expense. The Marine Surveys are also of immense benefit for the purpose of enabling our vessels to navigate the ocean in safety, and also to facilitate the selection of the proper points for the erection of piers and harbours, either by the nation or by patriotic individuals,—a noble example of which has been set by his Grace the Duke of Buccleuch and Mr Gladstone in the erection of the piers at Granton and Burntisland. It is much to be regretted that our Trigonometrical Survey has proceeded so slowly. The English Trigonometrical Survey commenced in earnest in 1791, the French Description Geometrique de la France, similar to ours, after the peace of 1815, and, considering their relative progress, the latter, though begun a quarter of a century after the former, it appears probable will be finished before it.*

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* Read before the Royal Scottish Society of Arts, 13th February 1843.

WILLIAM GALBRAITH.

54 SOUTH BRIDGE, *November* 1842.

In the Astronomische Nachrichten, conducted by Professor Schumacher of Altona, there are several papers by M. Bessel, the justly celebrated astronomer of Konigsberg, relative to the figure and magnitude of the earth. In this new determination, M. Bessel has thought proper to reduce almost all the astronomical observations afresh, and to introduce every necessary correction relative to the true places of the stars, the instruments employed in making the observations, and the latest corrections of the measured arcs. In general, however, he has not re-examined the trigonometrical calculations, but merely applied such corrections to the arcs as had been suggested by recent examinations of the standard scales from which the fundamental bases were obtained. This he has done with regard to the arc of the meridian in England, between Dunnose and Clifton, and it will be seen by the following remarks that the trigonometrical operations have, in general, been conducted with so much precision, that he was justified in the process which he pursued. The difference between my new calculations and those of the late General Mudge amount to a few feet only, when both are given in the imperial standard. Having come to this conclusion, it might be supposed that I need not have extended my remarks farther; but as some circumstances are alluded to that have not hitherto been observed, it may not, perhaps, be unnecessary or uninteresting to notice them. It is very probable that new computations have been made in the Ordnance Map Office nearly analogous to ours; but as they have not, to my knowledge, been yet made public, I may thus be excused for communicating mine.

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* It is now generally admitted, that, by a deflection of the plumb-line from the vertical in the direction of the meridian, an irregularity of 5″ of latitude has occurred at Arburyhill. But a deflection of the plumb-line in an arc at right angles to the meridian would cause a like irregularity in the azimuth there ; and if that azimuth be determined from the pole star, as obtained by Ramsden's theodolite, the irregularity from this cause must be multiplied by the secant of the altitude, or 1.68. Hence an error of 5″ committed at the height of the pole star becomes 5″ × 1.68 or 8″.4 at the horizon.

This hypothesis may be offered as a solution of the inconsistency pointed out in page 270 of this paper, independent of errors of collimation arising from a neglect to reverse the axis of the telescope.

I. OF THE BASES.

From the remarks made by General Roy himself in the Trigonometrical Survey, vol. i. page 15, &c., there can be little doubt that, of the first base measured on Hounslow Heath with glass-rods, the results were in Roy's own scale. Again, from the observations made by Mudge in p. 218, it is clear, I think, that the second base was measured in terms of Ramsden's scale. Now, the ratio of the first to the imperial standard is, from the best information we possess, 1.0000244 to 1, and that of the second as 1.0000691 to 1. The same applies to the bases measured on Salisbury Plain and Misterston Carr. From these data, the whole arc is readily converted into the imperial standard.

1. Roy's base, measured on Hounslow Heath, in terms of his own scale, at 62° Fahrenheit, and 100 above the mean level of the sea, was	27404.0137 feet.
Reduction of Roy's scale to the imperial standard = 27404 × 0.0000244 = . . .	+ 0.6699*
Length of base in imperial feet, ...	27404.6836
Log. of 27404.6836 feet, ...	4.4378248
Reduction for 100 feet of height to sea, . .	— 21
Reduction to chord, ...	— 0
Log. at level of the sea (1784), . . .	4.4378227

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* The ingenious writer of the article Trigonometrical Surveying in the Encyclopædia Britannica has applied this correction with a wrong sign.

2. Mudge's base, on Hounslow Heath, at 62° Fahr., 100 feet above the sea, in feet, ...	27404.3155
By Ramsden's scale, of which the logarithm is	4.4378190
Reduction to imperial standard, . . .	+ 300
Reduction to level of the sea, ...	— 21
Log. of true length of base (1791), . .	4.4378469
Which gives, imperial feet, ...	27406.076

3. Mudge's base, on Salisbury Plain, 690 feet above the level of the sea, at the temperature of 62° Fahr., was . .	36575.4 feet.
Of which the log. is	4.5631891
Reduction to imperial standard, . . .	+ 300
Reduction to level of the sea, ...	— 143
Log. of true length of base, ...	4.5632048
Which gives, in imperial feet, . . .	36576.723

4. Mudge's base, on Misterston Carr, 35 feet above the level of the sea, at 62° Fahr., was in the same standard,	26342.712 feet.
Of which the log. is	4.4206605
Reduction to imperial standard, . . .	+ 300
Reduction from 35 feet to level of sea, . .	— 7
Log. of true length of base at sea, . . .	4.4206898
Which in imperial feet is	26344.491

Hence we have the whole of these *four bases* all in imperial feet, with their corresponding logarithms, ready for the ulterior calculations, though the two bases on Hounslow Heath disagree more than is generally believed; because, instead of being, as usually supposed, in terms of the same scale, they are essentially different, and their lengths differ by 1 foot $4\frac{3}{4}$ inches.

Spherical Excess.

In computing the spherical excess, I employed the formula $\epsilon'' = \frac{1}{2} \sin 1'' \times 2\, a f^2$. In which a is the area of the triangle in square feet, f the factor to convert feet into arcs for $\frac{1}{3}(l + l' + l'') = \text{L}$, the mean latitude of the three angular points of the triangle at an angle α of 45° with the meridian. My results correspond nearly to those in the Survey, with one or two exceptions. That in which the error is greatest is the triangle Castle Ring, Bardonhill, Orpit. In this triangle, the spherical excess is $4''.06$ instead of $2''.85$, as stated in the

Trigonometrical Survey, vol. ii., Arc of the Meridian, page 53, No. XX. Here the error is — 1″.21, from an erroneous calculation. The differences in any other case are hardly worth pointing out.

In preparing the triangles for calculation, according to Legendre's method, by the *mean angles,* I have taken a fair mean of all the angles, whether by simple observation or by combination with others, without any arbitrary or empirical mode of proceeding according to any judgment formed of their relative accuracy in the opinion of the observers, as adopted in the Survey, being convinced, from experience, that such a method of procedure is often fallacious.

II. TRIGONOMETRICAL RESULTS.

With the bases and angles thus prepared, I found, from the bases, as measured on Hounslow Heath, the distance between Bagshot and Hindhead, forming one of the sides of the series of triangles constituting the arc of the meridian between Dunnose and Clifton. From the base on Salisbury Plain, I found the distance between Deanhill and Highclere, another of the sides of one of the triangles of the same series. Lastly, I found from the base measured on Misterton Carr, the distance of Clifton from Gringley, one of the sides of these series also. Whence I had the means of computing the sides of the whole series from four different bases, two values of that on Hounslow Heath, one on Salisbury Plain, and one on Misterton Carr—all prepared for calculation with the greatest care. I had also the choice of three methods of computing the length of the whole, two by parallels to the meridian, one on the east, one on the west, and a third by the intersections of the arc with the sides of the triangles.

From the base on Salisbury Plain I obtained

By the eastern series from Dunnose to Clifton, .	1036405.62 ft.
By the western series from Clifton to Dunnose, .	1036406.13
By intersections from Dunnose to Clifton, . .	1036405.58
Mean, ...	1036405.78
Correction for position of Zenith sector, . .	+ 3.00
Final results from Salisbury Plain base, . .	1036408.78

In all, the reduction of the distance between the perpendiculars to that between the parallels has been applied, a quantity entirely overlooked in the survey.

From a combination of the whole we have from

Roy's base on Hounslow Heath, .	1036361.06 ft.	$\epsilon_1 = -27.73$
Mudge's base on Hounslow Heath, .	1036418.83 ...	$\epsilon_2 = +30.04$
Mudge's base on Salisbury Plain, .	1036408.78 ...	$\epsilon_3 = +19.99$
Mudge's base on Misterton Carr, .	1036366.48 ...	$\epsilon_4 = -22.31$
Mean of the whole, . . .	1036388.79	

Now, dividing this mean by the length of the celestial arc 2° 50′ 23″ .497, and we have $1036388.79 \div 2^\circ.83986027 =$ 364943.58 feet, the length of one degree of the meridian at the middle latitude 52° 2′ 19″ between Dunnose and Clifton.

The value employed by M. Bessel agrees very nearly with our third result from Mudge's base on Salisbury Plain, and is consequently greater than our mean by about 20 feet,—a small difference in a distance of somewhat less than 200 miles; and the consistency of the whole is a proof of the great care and general accuracy of all the operations even at that early period. Whence the accuracy of Bessel's conclusions cannot in any appreciable degree be vitiated by the small discrepancy between the length of this arc, assumed by him, and that obtained by me from direct calculation. In short, he, by taking from the survey 1036337 feet, and multiplying this by 1.00007, Kater's number for reducing measures taken in Ramsden's scale to those in the imperial standard, obtained 1036409.54 feet, agreeing within less than a foot* of our third result, which, converted into French toises, was used in his subsequent investigations. Having thus proved that the results, in whatever way they be derived, are as accurate as could in general be expected, and that no error of consequence has been made so as to affect any conclusions deduced from the commonly received length of the arc of the meridian, I shall now proceed to make a very few concluding remarks.

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* Occasioned by the omission, in the Survey, of the reduction of the distance between the perpendiculars to that between the parallels.

III. GENERAL REMARKS.

Computing the co-ordinates of Leithhill from Kater's new survey, relative to the meridian and perpendicular to the meridian of Greenwich, and also those from the same point from Dunnose by the data given in the Trigonometrical Survey, I have found the difference of latitude, between Greenwich and Dunnose, to be geodetically . — 0° 51′ 34″.30 S.

Latitude of Greenwich by observation, . .	51 28 38 .50 N.
Latitude of Dunnose geodetically, . .	50° 37′ 4″.20 N.
Latitude by Kater's observations, . . .	50 37 5 .27
Latitude by Bessel's Z sector observations, .	50 37 6 .85
Mean of these three, ...	50° 37′ 5″.44 N.

which, from their close agreement, must be very near the truth.

From the same co-ordinates, the longitude geodetically, is, ...	1° 11′ 51″.56 W.
Longitude by Trigonometrical Survey, . .	1 11 36 .00 W.
Difference, ...	15″.56

or about 13″ in a degree.

This error arises from slight inaccuracies in the data assumed and methods then practised, but which, by Colonel Colby, the present conductor, have been long ago abandoned.

It is much to be regretted, indeed, that the later results and observations have not hitherto been published, for it would be very desirable that every thing connected with the Survey, like the astronomical observations made at the Royal Observatory at Greenwich, and other places, should be annually published at the public expense, since they are undoubtedly public property, and the results would enable civil-engineers, and private amateurs, to reap all the advantages justly expected from so valuable a source.

From calculation I have found the station at Clifton 4737.59 feet west of the meridian of Dunnose, when the arc is deduced from the azimuth at Dunnose; while, from the azimuth at Clifton, the arc passing through that station is 4909.95 feet west of Dunnose, or conversely, the station at Dunnose is 4909.95 feet east of the meridian of Clifton. Now, if all the operations and observations have been accurately performed, especially those to determine the azimuths, these numbers ought to be consistent; that is, they ought to be proportional to the radii of their respective parallels.

From this analogy 4737.59 feet become . .	5047.71 feet
But the calculation from the azimuths is, . .	4909.95 ...
Difference, ...	137.76 feet

Now, from Clifton, at the distance of Dunnose 137.76 feet, would subtend an angle of 27″.42. But the accurate determination of the azimuth by the pole star with the great theodolite, is an operation difficult to be performed in the manner described by General Mudge in the first volume of the Trigonometrical Survey, page 243. Captain Kater remarks in the new Survey for connecting the observatories of Greenwich and Paris, Philosophical Transactions for 1828, page 183: "There is, however, another source of inaccuracy to which azimuths by the pole-star are liable, and *which seems to have been wholly disregarded*—I allude to an error in the line of collimation. This error may, however, be destroyed by inverting the telescope, or placing that end of the axis which was on the east to the west; and taking a mean of the observations of the star in both positions." Certainly, if this inversion of the axis was omitted, a considerable error might ensue, as Captain Kater justly remarks; and though General Mudge does not directly say the axis was inverted, yet it is difficult to believe that so experienced an observer was likely to neglect it, though it is not impossible, unless he rectified it completely by the usual adjustments. But even though complete adjustment be attempted, yet the experienced observer will never implicitly trust to this, but will regularly invert the axis, as I am constantly in the habit of doing in all my determinations of angles, whether in altitude or azimuth. If this precaution, however, was really neglected, then it would seem to follow, from our computations above, that that error had amounted to one-half of 27″.42, or 13″.71, at each of the stations of Dunnose and Clifton. Indeed, from a computation which I have made, if the azimuth at Beachy Head be supposed correct, that at Dunnose would, by computation, differ from the observed quantity, by 13″.93; and conversely, if the azimuth at Dunnose be considered accurate, that at Beachy Head would, by calculation, differ from the observed quantity by 13″.93, or there is a probability of an error of 7″ in each, if considered equal. There is at least, certainly, some inconsistencies in these operations, for which it is difficult to account on any other hypothesis. The effect, however, on the length of the arc of the meridian, would be nearly insensible, though it might in some degree slightly vitiate other deductions, such as the latitudes, longitudes, and azimuths, dependent upon it. Indeed it may be remarked, that the peculiar construction of the old theodolite, by Ramsden, is not favourable to the accurate determination of azimuths by the pole-star. The altitude and azimuth circle, or transit instrument properly constructed, would, in my opinion, be greatly superior. A good altitude and azimuth circle, I believe to be the best instrument for determining the latitude; and the adoption of a small arc, as in the case of the zenith sectors, hitherto employed in this country, has always to me appeared not a little singular. A new zenith sector has lately been proposed by Mr Airy, with several improvements over the old instrument, which, if I am rightly informed, was destroyed by the late fire at the Tower. Still, however, though in the new instrument the angle be read on opposite arcs, yet it seems to be doubtful if its results can be considered equal to those from a circle of much smaller radius, read from *three* or *six* microscopes, distributed equidistantly round the circumference, when for every pair of observations it is reversed in azimuth, and the repetitions carried to four or six times within proper limits, and nicely reduced to the meridian. Indeed, notwithstanding the general excellence of the mural circle, as now constructed with microscopes attached to the stone pier for the sake of permanence, yet the circle itself being built up of so many different pieces liable to unequal strain, its execution is not entirely conformable to sound mechanical principles.* A transit circle, having both ends of its axis supported on stone-piers, must possess much greater stability, especially if made, with the exception of the axis, of cast-iron, with radiating bars, broad at the axis, and tapering towards the circumference on which the divisions are cut. The glasses of the telescope, too, ought to be much more substantially fixed to the circle than in the comparatively slender tube at present in use. The instrument would then be reversed by a proper machine in the same manner as the transit instrument; while, from the cheapness of the materials, it would be far less expensive.

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* It has little or no stability by braces in the direction of the axis.

While these general objections are made to English instruments, one would be justified in making still stronger to most of the foreign. The French repeating circle, invented by Borda, depends upon a principle of great ingenuity, though in practice it does not equal the sanguine expectations of its greatest admirers. There is a much greater want of stability in its structure than in any of our instruments, which, perhaps, might be improved by the suppression of some of their numerous adjustments; and though in the French arc of the meridian, and in the New Trigonometrical Survey of France, under the title, "Description Geometrique de la France," it has played a very important part; yet there are discrepancies in several of the observations connected with some of these fine operations, which would tend greatly to shake our confidence for extreme precision in its final results, deduced from even *thousands* of repetitions. In determining the latitude of the Observatory of Saint Martin d'Angers, as recorded in the Description Geometrique, Deuxième Partie, page 499, Colonel Coraboeuf, with a thirteen-inch repeating circle of Gambey, from observations on Palaris, at its upper transit *north* of the zenith, by about 40° 55′, found the latitude to be . . 47° 28′ 15″.21 N.
By α Serpentis, 40° 29′ S. of zenith, . 47 27 59 .41 N.

Half sum or mean, ... 47° 28′ 7″.31 N.
which is accounted the true latitude. But there is a difference between these results, amounting to no less than 15″.8, one-half of which, or 7″.9 taken negatively, is reckoned the error of the instrument at a zenith distance of about 40° 40′.

Again, by β Ursæ Minoris, at a zenith distance of about 27° 23′ N. at its upper transit, the latitude by the same instrument was . . . . 47° 28′ 10″.95 N.
By Arcturus, with Z. D., 27° 23′ S. it was 47 28 1 .41 N.

Half sum or mean, ... 47° 28′ 6″.95 N.

which is accounted the true latitude, and agrees very closely with the preceding result. There is, however, between the two last, a difference of 9″.54, one-half of which, or 4″.77, is here, at the zenith distance of about 27° 20′, reckoned the error. Hence, for a change of zenith distance of 13° 20′, there is a corresponding change of error of 3″.13. Some observers find, or think they find, that these errors vary as the sine of the zenith distance;* while others can detect no such law, though a mean of judiciously chosen observations give remarkably consistent results, when the observations are very numerously repeated. Still, however, in this country, observers accustomed to British instruments, would greatly suspect their final accuracy, even from *very numerous observations*, however consistent the individual results might be, whenever they involved such remarkable discrepancies. The opposite error seems applicable to our observers. Generally provided with large instruments, having powerful telescopes, they trust perhaps rather too confidently, in a very few observations which they consider good, and neglect to repeat them sufficiently to counteract atmospheric irregularities, for which no power of telescope will compensate. Even the power of the telescope of Roy's theodolite, by Ramsden, was not great, as he himself states, in the Trigonometrical Survey, vol. i. page 123; it only magnified about forty or fifty times, as commonly employed. I have not seen the power of that belonging to the Board of Ordnance anywhere stated.

In taking horizontal angles, " the errors," says Captain Kater, Phil. Transactions for 1828, page 197, " which may arise from lateral refraction, have often been suspected, but never clearly ascertained. In the course of our work, however, we had such evidence of the fact as to leave no doubt of its existence. The angle (measured) between the same objects would differ (when taken) under the most favourable circumstances, about *five seconds* on different days, and perhaps a second and a-half, or two seconds, may be considered as the error which may effect an angle from lateral refraction in an ordinary state of the atmosphere."

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* This coincides nearly with these examples, the difference being only 0″.78 from this hypothesis.

These remarks of Captain Kater have been verified by my own experience, and there is no probable way of obviating the effects of refraction on horizontal angles, but by combining the French method of repetition with our own more powerful instruments on different days under various atmospheric circumstances.

ADDITIONAL NOTE.

The following remarks have been occasioned by the receipt of a part of the Ordnance Survey, since the original paper was delivered to the Secretary:—

After a lapse of *thirty years,* the publication of the results of the Ordnance Trigonometrical Survey of Britain has been resumed. This has been recommenced by the publication of a part, titled, " Astronomical Observations, made with Ramsden's zenith sector, together with a Catalogue of Stars which have been observed, and the amplitudes of the celestial arcs, deduced from the observations at the different stations; and published by order of the Board of Ordnance."

Of this work a few copies have been distributed, by presentation, to different individuals, and it is but justice to those employed, to affirm, that all the deductions are made according to the best methods now used in that branch of science. Colonel Colby, the indefatigable conductor, has availed himself of the advice of Mr Airy, the astronomer-royal; and Lieutenant Yolland, of the Royal Engineers, under the Colonel, has followed up this advice with diligence and care.

The points of which the latitudes and intermediate arcs of the meridian are here given, are Dunnose in the Isle of Wight; Greenwich Observatory; Clifton Beacon in Yorkshire; Arburyhill in Northamptonshire; Delamere Forest in Cheshire; Burleigh Moor in Yorkshire; Kellie Law in Fifeshire; Cowhythe hill in Banffshire; and, lastly, the station on the small isle of Balta in Shetland, comprehending an arc of the meridian passing from the southern extremity of Britain, to the more northerly of the islets belonging to it, amounting to above ten degrees, or about one-ninth part of the quadrantal arc of the meridian from the equator to the pole. This will not only be a most valuable operation for improving the geography of the country,—a thing much wanted from the great inaccuracy of our maps and charts, but a valuable contribution also to astronomical and geodetical science. We are informed by the Colonel towards the close of his preface,—" That the terrestrial observations requisite to enable me," says he, " to complete and publish the geodetic distances connected with the astronomical results, are now in so advanced a state, that the printing of them will shortly be commenced." These being compared with others of a similar kind in different parts of the world, will enable him to deduce a proper value of the earth's axes, and thence to fix geodetically, with precision, the latitudes and longitudes of all the important points throughout the British Isles.

May all these important labours be speedily brought to a satisfactory conclusion, for the benefit of both agriculture and commerce, since, in the present state of our maps, the most palpable and dangerous errors, notwithstanding all that has been urged for their correction, still continue to exist, as will readily appear by an examination of the maps now submitted for inspection.

⁂ A few maps and charts were here exhibited, containing glaring and dangerous errors to navigators.

Description of a Portable Diorama, which may be viewed by a number of persons at a time. By George Tait, Esquire, Advocate, F.R.S.S.A. With a Plan. Communicated by the Royal Scottish Society of Arts.*

A portable diorama which I exhibited to the Royal Scottish Society of Arts in November 1841 and April 1842, and which was honoured with their medal, could be viewed by only one or two persons at a time, the pictures being *within the box*, and being seen through eye-holes.†

I have now made a diorama having the construction modified so that it may be viewed by a number of persons at a time, the pictures being placed *upon the front of a box*, where they are exposed uncovered. The front light is thrown upon them from without, and the back light from within, the box; and both may be increased or diminished at pleasure. Gas is the most convenient light; but oil may be employed, by adopting means for properly increasing or diminishing the light upon the pictures. The apparatus is used in a dark apartment; and ought to be so placed that the horizon of the pictures may be on a level with the eye. The effect of coloured sketches of a variety of changes which I made for the former diorama, is equally satisfactory in this.

The following side-elevation and plan represent a small diorama made upon this principle :—

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* Read before the Royal Scottish Society of Arts, on 23d January 1843.

† See the printed Transactions of the Royal Scottish Society of Arts, and the Edinburgh New Philosophical Journal for 1841, 1842.

Fig. 1.

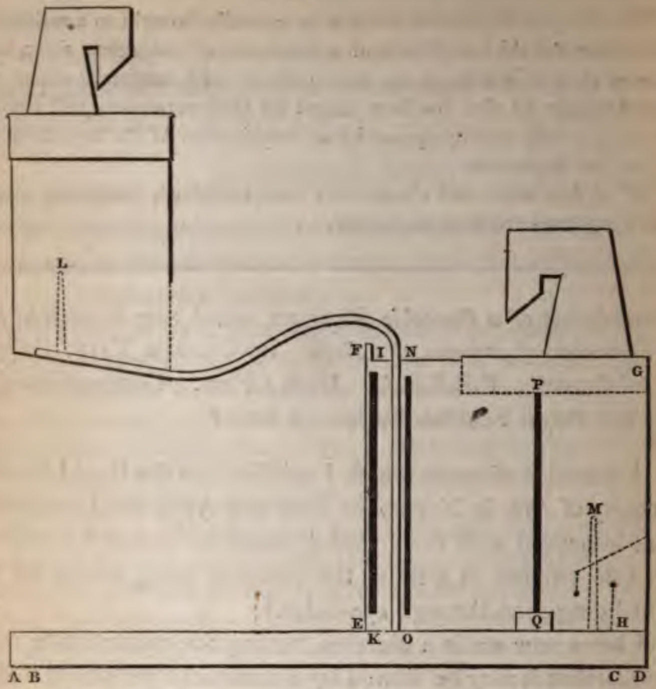
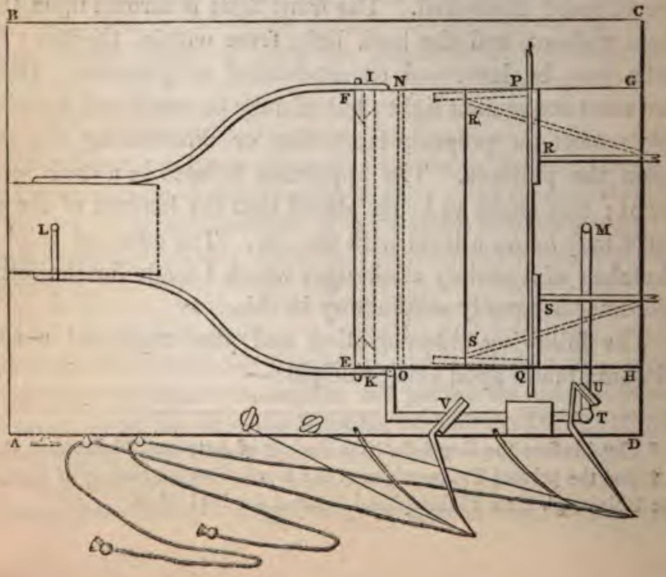


Fig. 2.



A B C D, a board to which the apparatus is attached. The length of the board is 18 inches, and that of the painted surface exposed, 6 inches,—but the larger the more striking.

E F G H, a box for receiving the pictures in front, at E F.

J K, opening in the side of the box, by which the pictures are introduced successively into a groove in front, behind a border of black velvet, to absorb stray rays from the front light. [In the former construction, as in this, the pictures may be conveniently entered at *the side* of the box. Both boxes may be made to receive the same pictures.]

L, front light, compact and bright, in a lantern constructed to direct and confine it to the pictures. If the flame be flat and have not a reflector or a lens, its edge may front the pictures. A simple swallow-tail burner, No. 0, gives sufficient light for this scale. The inside of the lantern is done with black japan, flat; and the sides and bottom, and outside of the bottom, and the supports, so far as necessary, are covered with black velvet.

M, back light. Swallow-tail No. 1, is sufficient for this scale.

A circulation of air is admitted to both flames without allowing the escape of light. Their covers are moveable, and are represented on the plan as removed.

N O, opening for receiving into a groove a slight frame of tissue paper, to be used when found of advantage; particularly, when any part of a picture, for example the moon, is made transparent.

P Q, opening through both sides of the box, for receiving into a groove an opaque slider, of a length equal to about double the breadth of the box, properly pierced, to be drawn gradually across, in order to represent passing gleams of sunshine; also for receiving a slider or sliders of tissue paper, painted with various tints in succession, to be drawn gradually across, in order to represent changes of tints, for evening or the like, with the back light, where day is represented by that light, as in fog or snow scenes. The light is not to be allowed to pass over or under those sliders.

When a slider is used, the tissue paper N O is to be removed; and the open space in front of the back light is to be contracted to about a third part of its breadth, by leaves moved forward for the time, as represented by R S on the plan, or otherwise.

A narrow projection immediately before any opening, if necessary, prevents the light within from being seen in front.

The box is white within, to reflect light.

T M, T L, on the Plan, tubes for gas (the latter consisting of one of the supports of the lantern, hollow), supplied, when in use, by inserting the nozle of a flexible tube at T, or otherwise.

U, V, stop-cocks moved by levers attached, which are closed by springs and opened by cords extending to the front. The levers have checks adaptable to the variable pressure of the gas, for example, linen threads attached to pins turning in the board, so that either flame, when not required, may be reduced to a blue point. The levers and springs are made to fold back upon the board when not in use.

The arrangement now shortly described is given merely as a specimen. The details of any diorama made upon this principle of construction, for example the description, number and position of the lights, will, of course, be adjusted according to the judgment of the maker, and will be modified by the size of the apparatus and other circumstances.

G. TAIT.

EDINBURGH, 2d *January* 1843.

Description of a Marine Salinometer for the purpose of indicating the Density of Brine in the Boilers of Marine Steam-Engines. Invented by J. SCOTT RUSSELL, M.A., F.R.S.E., F.R.S.S.A., Civil Engineer. (With two Plates.) Communicated by the Royal Scottish Society of Arts.*

It was very early in the history of steam navigation that the inconvenience of raising steam from salt water was experienced. When the Comet descended below Port-Glasgow in 1812, the boiler was found to boil over, or prime, as it is technically called by engineers, when part of the water is forced up so violently, along with the steam, as to pass over into the cylinder of the engine—a circumstance always detrimental, and sometimes destructive to the engines. This arises from the thickening of the water, its density being increased by the retention of the solid substances, which compose sea-water, and which remain and accumulate in the boiler, while the fresh portion of the water is passing off in the shape of steam.

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* Read before the Royal Scottish Society of Arts 28th February 1842, and the Honorary Silver Medal of the Society awarded 14th November 1842.

This process of accumulation of solid matter in the marine boiler is by no means slow. The whole of the water which a marine-boiler usually contains is evaporated in three or four hours, leaving the solid substances in the cubic content of boiler behind it, and being replaced by salt water, with an equal quantity of depositary matter, accumulating as rapidly as before; and since it is known the solid matter amounts to as much as $\frac{1}{40}$ of the whole mass of water, it would follow, if the process of ebullition could continue so long as 150 hours, there would be deposited in the boiler a quantity of solid matter equal to the number of tons of water in the whole content of the boiler.

Long, however, before this degree of solidification can take place, evils of a different description intervene to impair and put an end to the functions of the boiler. The solid constituents of salt water which are left behind do not diffuse themselves uniformly over the whole liquid mass, so as to constitute a homogeneous brine; on the contrary, the new supplies of sea-water, as they enter the boiler, remain secluded from the former more saturated brine, rise by their less specific gravity into an upper stratum, while the denser brine forms a bed in the lower part of the boiler, and surrounds the fire-box and heater-flues occupying the water-spaces and legs, which are usually at a high temperature, and which, in double-tiered boilers, are generally the most intensely heated. The intense heat of the metal expels the water from the brine in contact with it most rapidly in the hottest places, and salt is deposited on the hottest parts of the furnaces and flues, extending rapidly to those less heated, and so not only diminishing the evaporative power of the boiler, but injuring its substance, and endangering its existence.

The remedy for these evils was very early invented. But I have not been able to discover the inventor of the cleansing process commonly called "blowing down," or "blowing off." It is almost universal, and is performed in the following way:—There is forced into the boiler, at each stroke, rather more water than is required for the supply of steam, so that the boiler becomes too full. Openings are then suddenly made at the bottom of the boiler, and the brine at the bottom being violently ejected, carries with it any solid substances that may have accumulated near the bottom—the boiler is thus cleansed; and before the water has got too low, the openings are again closed, and the boiler continues to be fed as formerly.

Another remedy, pretty generally adopted, is the brine-pump, by which, for every portion of water supplied to the boiler, about one-fourth part of that quantity of brine is withdrawn from it. This process does not so thoroughly carry off all the impurities as the former; but it is attended with a saving of fuel by a contrivance for giving to the feed-water entering the boiler a portion of the heat of the discharged brine. The recent introduction of this process is due to Messrs Maudslay and Field of London.

In whatever way the saturation of the water with solid matter may be remedied, it is essential to the accomplishment of this object, that some simple apparatus should be contrived for the purpose of shewing when the cleansing process is required, and whether it is successfully applied. If this be not obtained, the usual consequence of acting on wrong data are sure to follow.

A contrivance was patented, which was thought promising, but was found liable to be mechanically out of order when most wanted;—a ball of greater specific gravity than salt water was connected with an external index, by which there was indicated on the outside the fact of the brine becoming sufficiently saturated to float this ball.

Another was to place in the glass gauge of the boiler a glass hydrometer bead, which would float when the brine became saturated to a given point, and fall to the bottom in the ordinary state of the boiler. But this fails entirely of accuracy, although very elegant, for the brine of which we wish to indicate the density is in the lower stratum, not the upper one, where the usual glass gauge is placed, and irretrievable mischief might be done before the indication would shew any change.

I have lately employed, in some large ships destined for transatlantic voyages, a species of brine-gauge, or index of saturation, which is found to possess every advantage, and which I therefore desire to communicate to the public through this Society. The drawings sent are such as may enable any engineer to construct them for himself. The details of the arrangement of the apparatus were made under the direction of Mr James Laurie, formerly one of my assistants; and he also has obliged me by writing out the annexed description of the operation of using the index.

The principle I have used is the well-known law, "that the heights of equiponderant columns of liquids vary inversely as the densities of those liquids."

If I take open glass tubes bent in the form of the letter U. as in the diagram (fig. 1), and pour one fluid into one of the sides, and another fluid into the opposite side (taking care to use the heavier liquid *before* the other); the one being mercury, and the other water, they will stand at the height of 1 inch and 13 inches respectively. If I use alcohol and water (fig. 2), they will stand at the height of 10 inches and 8 inches respectively, the height of the one fluid being always greater than that of the other, in the proportion in which its weight, density, or specific gravity is less.

Fig. 1.

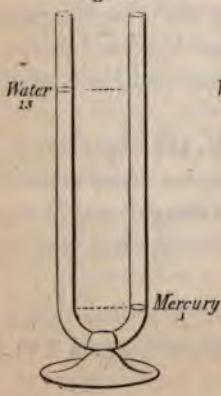


Fig. 2.

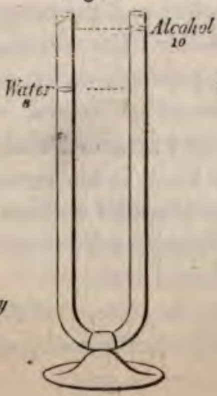
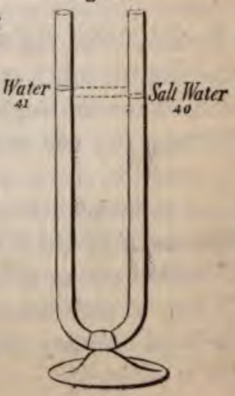


Fig. 3.



In like manner fresh water and salt water (fig. 3) will stand at heights of 40 and 41 inches, shewing a difference of 1 inch.

The use which I make of this principle is as follows:—I reckon the best scale of saltness of a boiler to be that which takes the common sea-water as a standard. Sea-water contains $\frac{1}{40}$ of saline matter. When the water has been evaporated, so as to leave only half the quantity of distilled water to the same quantity of saline matter, I call that two degrees of salt, or brine of the strength of two, and such brine would shew, in fig. 3, the columns 40 and 42, or double the saltness of sea-water, indicated by a difference of 2 inches. A farther saturation would be indicated by a difference of 3, 4, 5, and 6 inches between the columns, and so indicate three, four, five, six, and any further degrees of saltness—a range which may be made to any degree of minuteness by the subdivision of the scale of inches. This scale is that which appears to me most simply applicable here—and it is that which I adopt for marine boilers.

The mechanical apparatus which I have employed to give this indication is perfectly simple, and has the advantage of being such as the engineer already perfectly understands. To the marine boiler I apply two water-gauges of glass, instead of one as at present used; they both serve the purpose of the present glass gauges, and the pair would be valuable for this, if for no other reason, that there would always be a duplicate when one is broken, an accident not unfrequent. To these gauges I simply attach small copper pipes, so that one of them may be placed in communication only with the salt brine in the lower part of the boiler, and the other with the feed-water which is entering the boiler; the one then holds a column of brine, and the other of pure sea-water, and each inch of difference shews the degree of saturation.

Without the use of any attached scale, the engineer, by a little practice, comes to know in his particular vessel what difference in inches can be admitted without danger, and at what difference of height it is imperative to blow off. But it is convenient to have an attached scale.

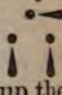
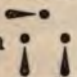
It may be satisfactory to state, that the practical range of scale in an ordinary boiler in the ordinary working, is 6 to 10 inches, a difference sufficiently great to be easily observed.

The rule of working them is nearly this :—Continue the operation of blowing off until, if possible, the difference of the columns is less than an inch, it will be unnecessary to blow off again until the difference is at least 6 inches.

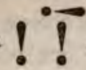
As a practical rule, I find that it is necessary to blow off when the brine at the bottom has about three degrees of saltness. But this will vary exceedingly, according as the construction of the boilers is more or less judicious. When the heat is greatest in the lowest portion of the boiler, and the flues return above, they will be most liable to salt, and require the most frequent cleansing.

The following is Mr Laurie's description of the instrument. The drawings give the details of the apparatus.—J. S. R.

The fact that the specific gravity of salt-water is greater than that of fresh, and that it increases with the degree of saturation, is what the operation of this instrument depends on ; by its means two columns of water, the one feed and the other brine, are poised against each other, so as that any difference of weight betwixt these columns immediately becomes apparent by the lighter of the two requiring an accession in quantity to resist the upward pressure to which both columns are subjected. This is accomplished by having two common glass gauge-tubes close together, each of which is connected with a separate tube ; that inside the boiler descends to the level of the water, the specific gravity of which is to be measured, and having either or both of these tubes so connected with the feed-pipe of the boiler, that by opening a cock one of the pipes will be filled with feed-water, while the other remains filled with brine, which cock being shut, the tubes remain so filled ; but inasmuch as feed-water is of less specific gravity than brine, it will be forced up and stand in the glass tube at a higher level than the brine, which difference of levels increases with the saturation—and hence the index to judge of the saltness.

In Plates VI. and VII., A, B, are the two glass gauge-tubes ; C, one of the tubes forming the connection betwixt one of these glass gauge-tubes and its tube D, that descends inside of the boiler ; E, the tube forming the connection betwixt the upper ends of these tubes and the inside of the boiler ; F, G, two cocks so made, as shewn in the drawing, that by their means each of the tubes inside of the boiler may be shut off from the glass tubes, and also may be connected with the tube H, leading from the feed-pipe of the boiler; I, a cock affording the means of shutting off the tube E from the glass tubes, and also of connecting either of these glass tubes with the tube K, leading to the bilge of the vessel; each of these cocks has a handle, and when the instrument is indicating, the three handles hang perpendicularly downwards. To bring the instrument into operation, the three handles must first be put in the position  which has the effect of allowing the brine to flow right up the glass tube A, and out through the tube K, into the bilge of the vessel; this having been done for so long a time as that A and its tube inside the boiler be thoroughly cleansed and filled with brine, the handles are then to be put in the position , which, in like manner, cleanses and fills B and its tube inside of the boiler with brine; finally, bring the handle of the top-cock into its original position, and put either of the lower handles horizontal, which forming a connection of the feed-pipe with one of the tubes inside of the boiler fills that tube with feed-water; thus there are in the two tubes inside of the boiler two columns of water of different specific gravities, the one being brine, the specific gravity of which is to be measured, and the other feed-water, the specific gravity of which is pretty nearly constant, so long as the temperature of condensation is the same, and does not vary much let the temperature of condensation be what it may; but, inasmuch as these columns of water are of different specific gravities, the pressure on the bottoms of them will force the lighter up the glass tube, until such a quantity of brine has followed it as makes it of equal weight with the other; and hence, in the two glass tubes, the water stands at different heights, the magnitude of which difference becomes known by means of the scale fixed betwixt the glass tubes, and therefore also the degree of saturation of the brine.

The use of this instrument, which might be called a Salinometer, is not confined to this one object, for it answers thoroughly all the purposes of the common glass gauge, the position of the surface of water in the boiler being midway betwixt the surfaces of water in the tubes.

When either or both of the glass tubes is broken, put the handles in the position , and nothing can escape from the boiler.

T. W. L.

Observations on the Llama, Alpaca, Guanaco, and Vicuna. By Mathie Hamilton, Esq., M. D., late of Peru. Communicated by the Author.

Of all the quadrupeds on the elevated regions of the southern American continent, the most worthy of notice is the Llama tribe, which includes the Llama, Alpaca, Guanaco, and Vicuna. The llama and alpaca are seen domesticated in Peru, but the guanaco and vicuna only in the wild state, except where they are kept as prisoners. When the vicuna has been kept within doors for a time, it becomes an interesting, frolicksome creature, but it never acquires the tame and docile habits of the llama or alpaca. A beautiful pet vicuna lived in the house with me for several months, and was in the habit of coming into the public room at stated times, and took bread from my hand, when it often jumped about in the apartment, and put itself in the most graceful attitudes.

VICUNA AND GUANACO.

The vicuna is much smaller than the guanaco or alpaca, and is more delicate and handsome in every respect. It has a large, prominent, glistening eye, which has a peculiar and expressive softness; and when running with amazing speed, its neck, which is long and slender, is carried in a curved position like that of a swan or the letter S. These creatures are exceedingly difficult to take without having recourse to artifice.

They are seen in small bands of a dozen or more, and are found chiefly in those uninhabited regions of the Andes, where vegetation is hardly sufficient to afford them a scanty subsistence. I never saw either a guanaco or a vicuna on the plain of Oruro, which is above 100 miles long, and about 12,000 feet above the sea; nor have I observed them on any part of the table-land of Bolivia. They were seen chiefly on the journey across what is called the Cordillera of the coast, which, travelling from Tacna or Arica to Potosi *via* Oruro, with cargo mules, requires 6 or 7 days before descending to the table-land, on which numerous flocks of llamas, alpacas, and sheep, are seen; but in the dismal region of the Cordillera the vicuna is found enjoying its freedom, and frequently indulging in its peculiar cry or rather whistle. It would seem to be ever on the watch against danger, for, when on the rout to Potosi, it sometimes happened, that on turning the shoulder of a mountain, or entering a ravine, I have seen a vicuna peep round a rock, or view us from an eminence, then immediately its whistle, not unlike that of the boatswain's, was heard, and a troop of vicunas might be seen bounding in the distance, setting at defiance pursuit.

It may be noticed, that the haunts of the vicuna appear to be confined to the more elevated regions of Peru only, for though in the higher lands towards the Equator, about Quito, we meet with the llama and alpaca, the vicuna is not found so far north, neither is it met with to the south beyond the tropic of Capricorn. It should also be noticed, that the same sort of food is used by all these species, and that which is most relished by them is called by the Indians *Ichu*, and grows to the height of several feet; it is a gramineous plant, and is called *Jarava* in the *Flora Peruana.*

No satisfactory reason has been given for the circumstance of vicunas being seen only within these latitudes. They are found on the elevated parts of the province of Santa Cruz de la Sierra, in the interior of Bolivia, near the junction of that state with Brazil; but they are not seen in the equatorial regions of the Andes, nor in Chili, nor farther south. It is possible that the greater altitude of the Punas of Peru, or Bolivia, where the atmosphere is drier and its pressure less, may be more congenial to the nature of this interesting animal than other parts of the Cordillera, such as about Quito, where there is more humidity, and several thousand feet less elevation. In some parts of those sterile solitudes frequented by vicunas, even the ichu does not grow; and in such places the mosses afford them a scanty subsistence.

In Peru, the guanaco haunts the same secluded tracts; but it does not mingle with the vicuna. The former is much larger and more powerful, and is found on the high lands throughout nearly 50 degrees of latitude, even to the straights of Magellan. The guanaco weighs, on an average, about 8 arrobas, or 200 lb., and it is much more easily caught or run down than the vicuna; though extremely shy and sensitive on the approach of danger, emitting a sound somewhat like the neigh of a horse, warning its companions, and then galloping off. Its skin is covered with a short coarse wool of a reddish-brown colour on the back and sides, running into stripes towards the belly, which inferiorly is white; and the neck, which is much stronger than that of the vicuna, is carried straight while it is running. Its wool is exported, and is used for domestic purposes. The wool of the vicuna is of a brown or fawn colour; and though it is shorter than that of the alpaca, yet it is much more valuable, being exceedingly fine and soft, so that articles made of it are very handsome. The real wool of the vicuna sells at a high price in Peru; and the best hats, gloves, ponchos, &c. are made of it, being more costly in proportion than the wool; but that may be a result of no spurious materials being put into the things manufactured there, and also from the difficulty of working such fine wool.

The city of La Paz, in Bolivia, is famous for the manufacture of hats; the finer sort are very well made, having a very broad brim, and are well adapted both for shading the head from the solar rays, and also from rain. In 1835, the price of hats in La Paz varied from one to fifty dollars each; one of the best, of vicuna wool, cost three doubloons, or L.10 sterling. Such a hat is soft and light, and may last many years. Ponchos are sometimes made of the same sort of wool, one of which costs more than fifty cotton ones, which for use serve nearly as well. The ancient sovereigns, the Incas of

Pèru, and their families, were clothed with the manufactured wool of vicunas; for the native Peruvians, and especially the females, in districts far in the interior, near the confines of Brazil, are expert weavers.

I have seen cotton goods of superior quality, such as table-covers, quilts, ponchos, &c. from the province of Moxos, but these were sold at a much higher price than similar articles from Europe. The late General Parroisien informed me that he had a poncho of vicuna wool, which cost 700 dollars, or L.140. There is reason to fear that now the vicunas will soon be exterminated, if those who have the power do not adopt measures for their protection, and prevent that indiscriminate slaughter which is now being inflicted on these interesting and valuable animals.

From time immemorial, the vicuna has been captured chiefly in the following manner :—A number of Indians form themselves into a *chaco,* or hunting party, together with some of those small dogs of which almost every family possesses one or more. They choose the proper time of the year, and, with a supply of corn and *chuno,*[*] resort to those dreary regions where the guanaco and vicuna are found. Having fallen in with their game, the Indians spread themselves over a wide extent of ground, accompanied by their dogs, and gradually narrow the circle. At a spot previously fixed on, there is a sort of enclosure made with ropes attached to poles brought for the purpose, and which are fixed in the ground at the necessary distances, and with the ropes at such a height as the pursued vicunas cannot pass with their heads elevated. On some occasions, to make the snare more complete, a wide space near the enclosure is surrounded by a number of small red flags, raised a little from the ground, and floating in the air.

The result is, that by means of the shouts of the Indians, and their gradual approach to the enclosure, with the barking and movements of the dogs, and the motion of the flags with the wind, the vicunas being naturally timid, are driven into the snare, and, neither jumping over nor stooping under the ropes, they are taken and slain, and skinned on the spot. In such excursions, the Indians in some cases are many weeks, and even months, in those inhospitable regions, away from the haunts of men, and at all times they suffer great privations. The cold is always severe during night, in consequence of the great altitude, and they are exposed to terrific storms of lightning and thunder, often accompanied with very large hail, or rather pieces of ice. When unsuccessful in the chase, they may be short of food and suffer severely.

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[*] Chuno is frosted potatoes in powder, and boiled in water with lard and spice into a sort of pottage, which is nutritive, and much used by the Indians.

Though such expeditions are called hunting, yet sport is not the object in view, but gain only. When the vicunas are captured, they are not shorn as in olden times, and then let go; but are killed, and the skins put aside with the wool on them; then the Indians gorge themselves and their dogs with the flesh, and if any portion of the carcase is left, the condors devour it.

In former times, the Indians were obliged by law to let all female vicunas escape after being shorn, and also the males, except a few which were allowed to be retained for food when necessary; and thus the continuance of the species was secured. But for many years past, an indiscriminate slaughter has been executed, and of course the number of vicunas is diminishing every year; and if stringent measures are not soon adopted to give protection, there is reason to fear that the race of vicunas will, ere long, become extinct, at least in so far as relates to the obtainment of the wool.

The reason assigned for flaying these creatures, instead of merely shearing them, is, that the wool is so valuable, that, when put up in bales, it is fraudulently mixed with other wool similar in colour, which in some cases is obtained both from the llama and alpaca; and, in these circumstances, merchants are not so willing to buy it. The government of Peru and Bolivia should immediately prohibit, under severe penalties, the destruction of vicunas. These animals might be shorn of their wool as in the time of the Incas, and as is done now with other wool producers in those parts, such as llamas, alpacas, and the common sheep, of which latter there are millions in Upper Peru.

LLAMA AND ALPACA.

The llama is at present found all over the southern tropic, from Rio Bamba at the foot of Chimborazo, under the equator, to beyond Potosi. It is a most important agent for the comfort and convenience of the Indian population of Peru. It affords food, and more especially clothing, and serves as a beast of burden; but it is not used for riding on, as has been erroneously narrated by some authors, for a Peruvian Indian never makes a journey on any animal, except when he is compelled to do so; and then it is on one capable of conveying more than 100 lbs., which is the maximum cargo for a llama or alpaca. It is not known when these creatures first appeared in those lofty regions where they now abound, but it would seem that they were in Peru prior to the appearance of the first Inca, Manco Capac, who reigned in the 12th century; for it is supposed that at an earlier period Peru was in the possession of a people who, though less advanced in civilization, we may conclude were in the habit of spinning the wool of these animals with the distaff; as, in the absence of written evidence, we find in their burial-places distaffs made of wood, indicating an earlier and a more rude state of society than that which existed under the Incas, whose subjects made their distaffs of copper, which have been taken from their huacas, along with the materials for spinning.

It is probable that these more ancient people availed themselves of the wool of the llama tribe for domestic purposes, and that the present race of Peruvians merely copied the example, or improved on the manufactures of their predecessors. Be that as it may, the llama and alpaca still exist in immense numbers all over the higher regions of Peru and Bolivia, and are a source both of profit and amusement to the natives.

None but those who have been on familiar terms with the Peruvian mountaineers, can know the deep interest which they take in their llamas and alpacas: they exhibit a solicitude in the welfare of these creatures, which seems to have other root than mere pecuniary considerations.

The Peruvian Indian is a mild, kindly being, when not under the debasing influence of ardent spirits, of which a great quantity is now annually consumed in the elevated districts. He is often insulated from neighbours and from his family while tending his flocks on the "ichuales," or on some long journey with them. In these circumstances, the Indian looks on his charge more as companions than as mere beasts of burden. I have often been amused to hear an Indian speak to a llama or alpaca as if it had understood him; and the plaintive instrumental music of the Indian, called yaravies, consisting of a succession of doleful and monotonous sounds, produced by blowing into one end of a reed, which is held like a clarionet, is supposed by them to be much appreciated by the llama. Those brutal acts of cruelty, which are so often inflicted on the dumb creation in some parts of Europe, are never imposed by the Peruvian on his fleecy charge; he rather adopts every means in his power to make them happy, and on a march with cargoes, he is ever on the watch to render assistance to a llama or alpaca whose burden may have shifted from its place, or where symptoms of weakness or weariness may appear.

Llamas, in their native clime, are on an average rather more than four feet in height from the spine to the ground, and the alpaca is a few inches less; but the latter is a much more handsome and interesting animal. There is a brilliancy and expression in the eye of the alpaca, as seen when on the punas of the Andes, which are not so striking to the observer who sees it on the coast only.

Indeed, there is a greater degree of vigour and vivacity in all the movements of these creatures when on their native soil, where the atmosphere is little more than half the density of that at the sea-level. The llama receives the male in the recumbent position, with its limbs doubled under its body, in the same manner as when asleep or at rest. Gestation continues seven months; one at a time is produced; it begins to breed the third year, and the duration of life is ten or twelve years. These animals are invaluable to the Indians of the Andes, who cannot afford to keep mules, even did the climate admit, but who, with a troop of llamas and alpacas, manage both to maintain their position in the social circle, and to save money when not plundered by the operations of contending armies. It would appear from the statements of some of the earlier writers on Peru, particularly Acosta, who wrote soon after the Spanish conquest, that llamas were then used for carrying silver from Potosi to Arica, on the coast of the Pacific Ocean, prior to its being shipped for Europe; but neither llamas nor alpacas have been employed for any such purpose during a long period, for the distance is so great, and the march of llamas so slow, as to make some other mode of transit necessary. Acosta states that the distance from Potosi to Arica is seventy leagues; hence it may be supposed that he never went over the ground, and that some of the earlier writers on Peru, like others of more recent date, often wrote without a competent knowledge of their subject, and drew on the imagination for facts alleged by them.

Of late years, much care has been taken to obtain more accurate information as to places and distances in Peru than can be had either from recorded statements or Spanish maps, most of which, either from design or ignorance, were often most erroneously given. The distance from Arica to Potosi, *via* Oruro, is 170 leagues, or 510 English miles, and the distance between the same places, by the Desert of Caranja, which is taking the hypotenuse of the triangle, is 154 leagues, or about 460 miles, by both of which routes I have travelled to Potosi and the coast. On the Desert route there is only one village seen, that of Andamarca, which is occupied by Indians, who speak the Amara language, and is seventy leagues from Potosi, and eighty-four from Arica or Tacna.

Llamas are not used for the conveyance of silver from Potosi to the coast; but the *tin*, which is obtained from the mines of Oruro, is brought to Arica by llamas and alpacas. The journey from Oruro to Arica, which is 100 leagues, takes one month with these creatures, for with burdens they travel only three or four leagues in twenty-four hours, and there are days of rest.

When a llama or alpaca is tired, he gives vent to his feelings by a peculiar cry, which is different from the sound which he utters when teased or irritated.*

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* These Indians believe, that if the cud or saliva which is ejected to a

If he is not allowed to rest, or relieved from his load soon after giving the notice of his weariness, he sinks to the earth in his usual peculiar manner, all his limbs being bent under his body, and there he dies. No kind treatment can induce him to attempt a renewal of the journey; and the Indians, knowing this singular characteristic of these animals, are disposed at all times to attend to their complaints, and to halt when necessary. It may be supposed that it would not be expedient to trust to such a mode of conveyance any thing of much intrinsic value.

The great motive which the Indian has to employ the llama as a beast of burden, is the total exemption from expense on the journey. They do not cost any thing for food or lodging; there are no tolls, and the Indian has his own necessaries carried by one of his pets, so that when one of them comes down to the coast with a quantity of tin bars or other goods, he both obtains a sum of money for freight, and also manages to sell some of his aged fleecy friends to the butcher to feed Indians resident on the coast.

No locality in Peru was more benefited by the llama than the city of Potosi during its greatest prosperity. When I was there in 1827, the population of that place was only 9000 souls, of whom only 1000 were employed in the mines; though so recently as in the year 1800, the population was 80,000, and at that time 20,000 men and boys were engaged in the mines and the works connected therewith. But it appears that about the year 1680 the population of the city of Potosi amounted to 160,000 souls, in consequence of the flourishing state of the mines at that time—for without these mines there never would have been a town or any inhabitants in a locality so very difficult of access as it is, and with such a horrid climate as is there experienced. However, it is wonderful what mercenary men will do to obtain the precious metals, and Potosi, to some extent, still stands a monument of the enterprise and perseverance of the Spaniards. A mint-house,

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distance by the llama when irritated, touches the human cutis, it produces *asrna* or itch, or, in the Indian language, *carache*. But though I have seen the experiment tried, I never knew a case of psora so induced.

larger than that at London, a palace, a theatre, court-houses, eighteen parish churches, and other public edifices, still testify what Potosi has been. This may seem to be a digression from the llama; but it is not so, for without the services of that animal, so well adapted to such an extraordinary locality, the mines could not have been wrought to the extent which they were. To understand how the llama was so necessary there, it should be stated that the cerro of Potosi, whence the silver ore is obtained, is at one end of the city, and all the works, where the ore is pounded, ground, roasted, and the silver extracted, have ever been at the other extremity of the town, and distant about a league from where the ore is brought up to the surface by Indians. All the ore is pounded and ground by means of machinery, acted on by water-wheels, which are moved by water from a very large reservoir placed among the hills above the city. The reservoir is supplied from various sources or ponds among the hills, whence the water is conveyed to the reservoir by means of aqueducts and conduits, and descends from the reservoir to the city by gravitation, supplying both the silver works and the town, many of the houses having the water conveyed into them through leaden pipes. But with all these advantages, the mines of Potosi could not have been wrought so easily without the aid of the llama and alpaca; for the ore, in immense quantity, had to be carried from the mines to the works, and that, too, over a most rugged and unequal surface, at an altitude of nearly 14,000 feet above the level of the ocean: no other animals in the world are so well adapted for such work in such a locality. Except water, every thing for the sustenance of man and beast has to be brought to Potosi from a distance of many miles over mountain tracts, the nearest spot where fuel (wood or charcoal) is obtained being thirty miles off. In such circumstances, the llama was invaluable, its food, pajon, *i. e. ichu* in the dry state, was brought by means of mules and asses, so that these llamas or alpacas cost very little for maintenance while working at the mines. The result was, that many thousands of them were employed in Potosi as beasts of burden between the mines and the places where the machinery is placed; and, when necessary, the flesh was used for food by the vast Indian population of Potosi, while the wool was made into warm clothing, so necessary in that rigorous climate, where at night the temperature is below the freezing point, though, during the day, the solar rays are often noxious to health.

The number of llamas and alpacas in Bolivia and Upper Peru is still very great, amounting to several millions, and the common sheep is also abundant. From the milk of the latter good butter is made by the Indians, but is little used by them, it being mostly put into bladders and sent to places where a good price is obtained for it; cheese is also made from the same source. The common sheep there affords a large quantity of wool; and if proper means were adopted, the number of llamas, alpacas, and sheep might be increased, and, of course, there would be a corresponding amount of fleeces. The Indians are not much in the habit of slaughtering the llama or alpaca for food so long as they are otherwise useful; the sheep and lamb are oftener used for culinary purposes, and *white* men seldom wish to eat llama-flesh a *second* time if they can get anything better. None of those animals require the use of tar or any unctuous substance while on the punas of Peru.

The climate on these heights is very peculiar, for though during a part of the year there is much rain or snow on the western slopes of the Andes, and occasionally where the llama and alpaca are mostly seen, yet the air on the punas is singularly dry, so that a want of perspiration, even among the human species, is a general complaint there. "No puedo yo a sudor," is often heard.

Thus, the climate where these animals thrive so well, is very elastic, and the reverse of damp or humid, which circumstance, together with the sort of food they get, and the exceeding rare atmosphere in which they live, may be the cause of the fine fleece obtained.

When I was in Bolivia much ignorance and carelessness was shewn by most of the proprietors of flocks relative to the management of the wool, which, in many cases, was allowed to drop, or to be torn off, and was not shorn at stated periods, as should be done under a proper system of management.

But latterly there has been such a demand for wool that more attention will be given to the fleece, both of the llama tribe and the common sheep of Peru; and if this important object be taken up by competent parties, both the quality and quantity of wools from that quarter may be increased.

It has been suggested that attempts should be made to naturalize the alpaca and llama on a large scale in this country for the purpose of wool-growing, and also for obtaining the flesh of these animals to eat; but as to the latter, not to notice its cost, the important question arises, would the flesh of these creatures be relished by people in Britain; and though *I* have no desire again to partake of such '*venison*,' yet the experiment may now be made, seeing that a number of these animals are now domiciled in this country; and as tastes do differ, it is possible that a joint of a llama or alpaca may become a welcome dish on the Englishman's table.

But allowing the eatability of alpaca flesh among Englishmen, another question arises, would it be *profitable*? and also, can the wools of these animals be purchased at a much cheaper rate when sent from Peru than they could be bought at, if purchased from the speculator in llamas or alpacas, who would propose to rear them on a grand scale on the bogs and sterile mountains, or other parts of Britain and Ireland? These are important points for the consideration of all who would involve capital in such a speculation. I still hold the opinion expressed at the meeting of the British Association at Glasgow, *i. e.* that the experiment is worth trying by those who are able and willing to risk the necessary expense; but I fear that it cannot succeed, because, besides other adverse circumstances, the climate of Britain is very unlike that of the native country of the alpaca.

It may be noticed that many llamas and alpacas are altogether white, but more of them, especially alpacas, are wholly black, exhibiting as marked a contrast as the black and white swine which are seen in Piedmont.

Party-coloured llamas and alpacas are numerous; and wool from them of a brown colour has occasionally been mixed with that of the vicuna.

The Indians of the mountains manufacture themselves nearly all their warm clothing from the wool of their animals; and so many being all black, they are able to appear in dresses of a sable hue without the aid of a dyer; and numbers of them of both sexes are dressed in black garments, which circumstance has induced some persons to suppose, that the Peruvians of the present day are still in mourning for their Incas; but the true explanation is the fact just noted.

From the wools of different colours, fancy pieces are also made by these Indians, whose mode of weaving, in so far as I saw it, is primitive in the extreme. When passing through the village of Andamarca, I observed a woman weaving a piece of black cloth: her loom was composed of only four short bits of wood, which were stuck into the ground in the open air before her hut; she was resting on both her knees, and stooping at the work, and conveyed the weft from one side of the cloth to the other with her fingers—the piece appeared about 18 inches in width.

A few years ago, there was no fixed price in Bolivia for alpacas, &c., for that varied with the locality and other circumstances. In 1827, when on the route from Potosi to the coast, through the desert of Caranja, we were under the necessity of occasionally buying a sheep or llama, for we travelled with a number of mules loaded with silver, and were seventeen days on the journey. We passed some numerous flocks of llamas, alpacas, and sheep, and though not a human habitation was seen throughout one portion of the route of above 200 miles, yet, as was stated by our guides, all these creatures had owners who would miss any which might be taken from their flocks. While on the march one day, our cook first ran down with his mule, and then picked up a sheep from a herd, for which he had not paid, as no person was in sight; but after we had travelled four hours, or above twelve miles from the spot where our mutton was obtained, an Indian overtook us and held out his hand for "quatro reales," 2s., the price of the sheep, and was quite happy with his half dollar, though he had to trudge 24 miles for it; at the same time I learned that while half a dollar was the price of a sheep there, that of an alpaca was a dollar, and two dollars that of a full grown llama.

In some parts of those vast solitudes between the eastern and western Andes, there is no vegetation of any sort, but at other places the ichu grows in abundance, and there myriads of llamas and alpacas are seen, thriving in their native but rigorous climate; and exhibiting a length of fleece (in some cases not shorn for years) which would astonish an English wool-stapler. In these deserts water is rarely seen, except at some of the halting stations, where a hole dug in the ground affords a supply of bad quality. I never saw a llama or an alpaca take a drink.

The price of llamas on the coast of Peru varies at different times and places. At Tacna, in 1835, the price was three or four dollars, and I never knew more than six dollars being paid for those which were shipped for Europe. When we consider the expense of conveying these creatures from Peru to England, it is obvious that it will not be profitable to obtain wool from the animals so imported; and it has been already stated, that an attempt to rear them in this country, in sufficient numbers, is not likely to succeed.

On the Existence of raised Beaches in the neighbourhood of St Andrews. By R. Chambers, Esq., F.R.S.E. With a Plate. Communicated by the Author.*

On coming, in May last year, to reside in St Andrews, I was much struck at the very first by certain geognostic features of the environs, of the same character with those remains of ancient beaches which have excited the attention of geologists in other parts of the island, but much more distinct than any which I have had an opportunity of seeing. Afterwards, as I extended my rambles from St Andrews, I was much interested in observing continuations of these remarkable platforms along towards the vale of the Eden, some way up that vale, and on the country immediately beyond it. It seemed to me that St Andrews presented unusual opportunities for the study of this particular class of geological phenomena, and that it might be worth while to direct local attention to these geognostic features, as many young persons, and others who had not given much attention to geology, might thus be enabled, at the cost of little more trouble than that of a forenoon's walk, to study what is certainly one of the most curious and wonderful results of geological research and speculation which have been laid before the public for some years.

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* Read before the Philosophical Society of St Andrews.

The particular superficial feature which first arrested my attention in this neighbourhood, was the platform on which the town stands, with its smooth continuation westwards to Lawpark, and north-westwards to Strathtyrum. The uniform linearity of this piece of country is such as might strike the most careless eye. I also observed that, to the south of the Kinness Burn, there was a continuation of this platform on exactly the same level—a vale of from an eighth to a quarter of a mile intervening. It was not long after, that I found a narrow stripe of the same platform extending beyond Strathtyrum, towards the Guard Bridge, and traced, what appeared, its continuation in Leuchars parish, north of the Eden. I also could plainly trace, on the ascent towards Scooniehill, a second or higher platform, less extensive in all respects, but equally linear and level. Finally, I have found fainter traces of a third, and even of a fourth platform, the last being the narrow stripe on which Mount Melville House and Feddinch Mains are situated.

To speak particularly of the first plateau. It may be described as a slope of very slight inclination, rising from the verge of the sea between St Nicholas and the Butts, towards Lawpark, and extending westwards to the site of Bloomhill and Kincaple. The town of St Andrews is situated on the part nearest the sea. But for the deep and wide intersection formed by the Kinness Burn, and a few similar but smaller intersections, it would have been a still more remarkable tract of linearly surfaced ground. The soil, I am told, is generally of a sandy character, such as might be expected on a tract like Leith Sands, or the West Sands of St Andrews, if these beaches were to be raised above the sea-level, and transformed into dry land. It is observable to every eye, that scarcely any stones occur throughout this tract: the fields everywhere seem composed of a light powdery earth, and the site of the town itself is so sandy, that rain never rests on the streets for any length of time.

The second plateau is a comparatively narrow terrace, traceable on the hither face of Scooniehill, and for a considerable way to the eastward, generally about a hundred feet above the level of the first plateau. I have chiefly observed it opposite to the town, but I learn from Mr Duncan, land-surveyor, that it is clearly traceable along by Brownhills farm, and as far eastward as his own house at Thornbank, three miles from St Andrews. Its western extremity melts into the slope of Scooniehill, at a point a little to the westward of Pipeland farm-house, which is situated upon it.

What I think may prove to be a third plateau is the generally level piece of ground on which Ballone and Lumbo farm-houses are situated, and which extends a little to the eastward of Cairnsmill, overlapping (so to speak) the western termination of the second terrace. Cairnsmill is situated in a hollow of this plateau, wrought by the rivulet which passes it on its way to join the Kinness Burn. I have paid less attention to the fourth plateau, but deem it also tolerably distinct. As mentioned before, Mount Melville House and Feddinch Mains farm-house are situated upon it. It seems to be less elevated above the third than the third is elevated above the second, or the second above the first; but, on this point, I only speak by the vague information of the eye.

From what I had previously seen of the ancient beach along the Firth of Forth, I had, of course, no doubt as to the original character of, at least, the first and second plateau at St Andrews; but, as many here, from unacquaintance with the subject, might be unprepared to see the matter as I saw it, and for the sake of accurate information for myself, I resolved to have the levels along these beaches taken by an unprejudiced and professional hand. Mr Duncan has done me this service in a highly satisfactory manner. It must here be remarked, that to take these levels is a very delicate matter, for the plateau is in so many places cut down, or worn away, by rills, that it is difficult to pitch upon spots which may be presumed to be near the line of the original surface. When you stand, indeed, upon the plateau itself, you are apt to be confounded by the undulations which you see near you, and it is when you take a somewhat distant view that the linearity is most striking. There is another effect of time which adds to the confusion, namely, the wearing down of the ancient sea-cliffs above and below, which tends to give the sectional line only a slight wave in some places. It was necessary beforehand to pitch upon places which, at a distant view, seemed unworn by the intersecting rills, and to follow a line sufficiently distant from the ancient sea-cliff, to be unaffected by its debris. Mr Duncan did his best to walk by these rules; but he could not be expected, in the circumstances, to work out my wishes with perfect exactness. We must also be prepared to allow for slight discrepancies, on account of presumable slight inequalities in even the original line of the ground. Every here and there, along such an esplanade as the West Sands, we may observe slight swells and depressions of the surface. Besides, an uniform degree of elevation is not predicated in the case; a general linearity within a considerable space or tract, is what we may say is looked for by the geologist.

The accompanying map (Plate VIII.) contains Mr Duncan's marks along the lines of the first and second plateau, namely, ten marked levels in the first instance, and nine in the second. Beginning with the first plateau at an interesting crust of it which overhangs the eastern extremity of the East Sands, he goes westward in a curving line to the south-east corner of the Strathtyrum policy, near Balgove, giving the following levels in succession:—60, 62, 65½, 68½, 70, 68½, 70, 74, 69, 74. The sixth of these numbers (68½ feet) is given at the spot where the line crosses the Largo road. The eighth of the series (74 feet) is given near Lawpark. I may here observe, that Mr Duncan takes, as a datum line from which to mark his levels, the high-water mark, as presumed to be indicated by the abutment of the arch which crosses the Kinness Burn at St Nicholas. He has also found by the spirit-level, that the gently sloping table-land behind Easter Kincaple is on the same level with the ground immediately south-east of Strathtyrum. Indeed, the identity of surface line which exists between Strathtyrum and Kincaple is remarkable to the unassisted eye, and forms a phenomenon which it would be impossible at present to account for otherwise. Mr Duncan's marks on the second plateau are equally striking, from not only a uniformity in themselves, but a uniformity in relation to the first plateau. Commencing here to the eastward of Kingask, and following a curving line westwards to the termination at Pipeland, he gives the following series of numbers, expressive of the height of the various parts above the present high-water mark:— 156, 154, 154, 157, 161, 156, 155, 170, 166. The extreme variation here is 14 feet; that between the first and last number only ten, the places pointed to being several miles apart. As in the first plateau, the increase of height is towards the west or *inland*.

Mr Duncan has made some further observations, not by regular *levelling*, as in these instances, but by his eye only and by the use of the telescopic spirit-level. I here quote from his notes:—" Taking up the first old beach where we left off near St Nicholas, we have first (going eastward) a break of about a mile, caused by the steep cliffs and high bold shore under Brownhills. Passing, however, a little to the east of Kittock's Den, we again come upon land exactly suiting our level, and answering, not only in this particular, but in every other, the character of an old sea-bed. This almost level surface, I followed out for several miles, with no interruptions but what were perfectly explicable. Where I left off, the same gently sloping land was continued onward, and I have no doubt that it would be found to go all the way round above Fife Ness, and for a considerable way up the shores of the Firth of Forth. The soil, almost everywhere throughout what I have inspected of this ancient beach, is of a like nature, being light and dry, and full of small shells, and of excellent quality."

With respect to the country beyond the Eden, he states as follows: " Commencing my levelling from what was pointed out to me as being nearly about high-water mark, on the

Mottrey Burn, at Milton Saw-Mill, I passed by Milton farm-house, ascended the hill or steep bank northward, and continued along its flattish ridge, as far as the small round hollow near its northern extremity. In passing along, I determined the elevation of the under-mentioned objects, principally by directing the telescopic sight of the spirit-level towards them, and making the necessary allowances for dip, &c. This taking the height by observation, it may be remarked, cannot be depended upon within a few feet:—

	Feet.
1. Elevation of a rounded bank on the Dundee Road, opposite Milton farm-house, ...	50
2. Ground on which Leuchars church stands, ...	55
3. A flat bank or surface extending from the west end of Leuchars village northward, along the left of the Dundee Road, ...	56 to 60
4. A high flattish bank to the south-west of Milton Saw-Mill,	62
5. A flattish gently-sloping surface, north-east of Pusk (average), ...	60
6. Height of Milton farm-bank, near its south end, . .	72
7. The same, at the north march of the farm, . . .	91"

Mr Duncan found some other platforms in this neighbourhood, which are generally about 107 feet above the level of the sea. This, it will be observed, does not correspond with any beach observable near St Andrews; but this may be accounted for in various ways—by none more simple than this, that that beach may not be marked in our immediate neighbourhood. As our second plateau is not marked on Scoonie-Hill, west of Pipeland, so may this not have been marked in that situation at all. The other elevations enumerated by Mr Duncan, correspond strikingly with those of our first plateau. The remarkable-looking mound on which Leuchars kirk stands, is composed of gravel and other sea-deposited materials. It is clearly a fragment of the last sea-bed, left by accidental causes. Mr Fraser, in his Map of Fife, gives its height as 57 feet, which is just about that of a large part of the platform on which St Andrews is situated. The linearity of the surfaces enumerated by Mr Duncan in the Leuchars district, is extremely striking; and from that place, the lines formed by our own second, third, and fourth terraces, are seen with the greatest distinctness.

Applying the theory of upheaving forces to our vicinage, we must presume, that at one time—a time early as compared with our historical retrospect, but late in geological chronology—certainly later than any of the trap disturbances, or even the age of the diluvium—only the tops of some of the neighbouring hills were above the surface of the sea. The sea then closely surrounded the heights on which Scoonie-Hill farm-house and Feddinch House are situated, and the Drumcarrie Hill. It was at that time that the platform on which Mount Melville house and Feddinch Mains stand, was formed. An upheaval, to an extent which I am not able at present to specify, raised a larger portion of the slopes of those and other heights into the air, and then began the formation of the second platform—that on which Pipeland and Old Grange are situated. Another upheaval, of about 100 feet, extended the bounds of dry-land still farther, and then began the formation of what I have called the first or great plateau. This may be presumed to have been, in our locality, an extensive sandy-beach, much like that now existing at Leith. The tide must have every day risen and fallen at least a mile, namely, along the ground now covered by the town, and up to the site of Lawpark Cottage, where the traces of the beach terminate in that direction. Afterwards an upheaval of about 60 feet must have taken place, leaving land and sea in what, generally speaking, may be called their present relative situations. The last beach was now dry land. At the site of the town and to the eastward, the ocean rested upon the upturned edges of a series of the lower carboniferous strata, which, in time, it seems to have cut down into the present beach and overhanging cliffs. To the westward of the site of the town, where these sandstone strata ceased to appear, the sea rested against a bank of clay, which it, in like manner, cut in upon; this is the bank which now sweeps round from Pilmour Row, by Strathtyrum, and along under Bloomhill and Kincaple, to the Eden. At the one place, there was a promontory; at the other, a bay. But as the rocks were worn down at the one place, the bay was filled up with sand at the other. This effect the waves and winds would conspire to bring about. We must not, therefore, be surprised to find the Links of St Andrews, and the whole ground under the Strathtyrum bank, several feet above the level of the sea. The whole of that land is one mass of sand, the lower part of which is probably of aqueous deposition, while the upper part is evidently an accumulation effected by high winds blowing from the sea, after the manner of many similar accumulations in other parts of the world, aided, perhaps, by occasional tides of abnormal height. Towards the mouth of the Eden, another cause comes in to help this formation, namely, the silt brought down by the river. The Tents' Muir, to the north of the mouth of the Eden, is an accumulation chiefly of wind-blown sand, like the Pilmour or St Andrews Links.

Both the beaches and cliffs have here, as usual, been much cut by water-courses. We have a cut on each side of Mr Brown's house of Grange, one on each side of Pipeland farmhouse, a great one in the line of the Kinness Burn, and several others. The vale of the Kinness Burn, below Lawpark, has all, of course, been formed since the last upheaval, and it is easy to see why it has taken the direction which we find it has taken. The spot at Lawpark has been the bottom or terminating point of a small bay, where the rivulet was originally received. The direction of this bay was towards St Nicolas, or the site of the present harbour; that is to say, a line between these two points ran over a somewhat lower part of the beach than the rest. Along this line, the rivulet would proceed at the ebb of tide. After the upheaval it would begin to cut down into its original sandy channel; and this process would be continued till, with its small accessories, it had carved out the present little vale between the site of the town and the opposite bank, nearly (in some places) a quarter of a mile distant. But for the formation of this vale, and the rearing of the town, we should have had at this place a piece of ancient beach clearly perceptible to the eye, of an extent which I have never seen equalled.

The apparatus brought before the society in connection with this paper, is an humble attempt of my own to illustrate sensibly the upheaving power and its effects. A model in putty of the country near St Andrews, is formed upon a flat plate of iron, which is suspended in a trough partially filled with water, so as to leave the supposed Mount Melville beach on a level with the surface of the water. By mechanism, the plate can be raised till the third beach is brought to the same level, next the second, afterwards the first; and, finally, by a further elevation, land and sea are shewn in their present relative situations, excepting that I have represented, as already formed, that sandy embankment which now keeps the sea most part of a mile away from the Strathtyrum bank.

Brief remarks on the Expediency of Forming Harbours of Refuge on the East Coast of Scotland, between the Moray Firth and the Firth of Forth.[*] By JOHN FLEMING, D. D., Professor of Natural Philosophy, King's College, Aberdeen, F.R.S.E., Member of Wernerian Society, &c. Communicated by the Author.

The subject of the following observations appears to be well calculated to command public attention, whether we consider the amount of human life, or the value of commercial property at stake. That no public enquiry should have been instituted respecting the exposed state of the East Coast of Scotland, with a view to the formation of HARBOURS OF REFUGE, when it was granted elsewhere, may seem inexplicable, unless we bear in mind that lamentable apathy exhibited by our representatives in Parliament, wherever Scottish interests of a *general* character are concerned.

The necessity which arises for the construction of harbours of refuge, involves the consideration of the *defects* of the existing harbours, which have been so long resorted to, and which at one period of our trade might have been deemed sufficient for every ordinary purpose. But to comprehend the true character of these defects, it is necessary to advert, however briefly, to a few elementary truths of physical geography, which may not perhaps be generally attended to, although highly illustrative of the subject.

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[*] The substance of the remarks on Harbours of Refuge, was communicated to the Aberdeen Philosophical Society, at their first meeting, February 7. 1840.

When we examine a VALLEY of any extent with the eye of a geologist, we shall generally find that the rocks which exist in the trough, are softer and more easily acted upon than those which form the bounding ridges. Interspersed portions of harder rock may be occasionally found among the softer materials, but those will merely cause inequalities in the valley, and mark, by their elevation, the resistance which has been offered to the disintegrating forces which have reduced the contiguous portions to a lower level.

When we examine a BAY, or indentation on the coast, we generally find analogous appearances. The softer beds have been acted upon, broken up, and removed by the action of the *ripple* or wind-waves; while the harder materials remain and constitute those promontories or *nesses*, which form the lateral limits of the recess or creek. Even in the bay, as in the valley, certain portions of harder rock may have existed, and such will usually be preserved as islets or skerries, to mark the abrasion which has taken place around.

If, then, the softness of the strata be the primary condition which gives rise to valleys and bays, we may expect to find in general a valley, on reaching the sea-shore, terminating in a bay, while a bay will be a tolerably sure indication of a landward valley. Several rather interesting examples, in illustration of these statements, may be observed in this immediate neighbourhood.

The bay of Aberdeen, with its lateral *nesses* of gneiss, seems to have been excavated in a deposit of old red sandstone, several patches of which occur in the neighbourhood, and attest its former more extended distribution. The bay of Nigg, with similar lateral *nesses*, appears to have been produced by the yielding of soft strata of mica-slate. The bay of Stonehaven has been excavated in comparatively soft strata of grey sandstone, with its northern ness of compact mica-slate, and its southern ness of old red sandstone conglomerate. To the south of Stonehaven, and in the neighbourhood of the ancient and celebrated Dunottar Castle, several small bays may be observed, deriving their origin from the beds of soft grey sandstone which alternate with the conglomerate, and the latter being less destructible by the action of the sea, forms the bounding nesses, aided in a few places by amygdaloid or porphyry. In general, indeed, it will be found that the observer of nature can seldom traverse any considerable portion of the coast without, here and there, meeting with sandy beaches, at the margin of bays, where all traces of the rock have disappeared, and he may consider himself fortunate if he succeed in detecting the solid materials he is searching after, at low water-mark, or in some inland ravine.

The valleys necessarily form the recipients of the rain water, and constitute river basins; and the rivers thus formed by them, and flowing through them, serve, in turn, to augment their capacity, by carrying to a lower level the disintegrated materials which have been produced by atmospheric influence. These materials become accumulated at the junction of the river with the sea, and constitute, in certain cases, those *deltas* which frequently occasion a subdivision of the main stream.

The disintegrated materials of the bay, associated in some places with those of the valley, and which usually consist of sand and gravel, are employed in forming the sea-beach. The irregular but almost constant action of the *ripple* or wind-waves, produces a uniform distribution of these materials, and as certainly restores the breach which disturbing causes may have produced in its continuity.

These materials, thus exposed to a ripple action variable in its intensity and direction, are usually arrested in their progress by the *nesses* which limit the bays, so that the character of the beaches of two contiguous bays may differ considerably from each other. The beach of Aberdeen bay, *e. g.*, is sandy, while that of the neighbouring bay of Nigg consists of very coarse gravel.

When a river, on its way to the sea, reaches a bay with its margin constituted as we have been describing, it has to maintain a constant warfare with this tendency to continuity of the sand and pebbles of the beach. If, during a flood, the river has succeeded in forcing a passage, and in making for itself a channel towards low water-mark, this new course becomes exposed to ripple action, and will be speedily obliterated to a certain extent, whenever the quantity and velocity of the water become reduced. This is strikingly illustrated in the condition of many of the rivulets which empty themselves into the bay of Aberdeen, to the north of the river Don, and may be observed with but little modification in the Don itself.

In general, therefore, it will be found, that when a river, after traversing a valley, falls into a bay of the sea having its shore covered with moveable materials, it has to contend with this character of the beach, to have its contents continuously distributed, and hence a *bar* must be formed of the materials carried into deeper water, while its distance from the shore will depend on the weakness or strength of the stream, and its shape be modified by the currents of the passing tides.

The banks of rivers invite the settlement of a population, from the superior fertility of the soil in the neighbourhood, the accompanying shelter, and the supply of water for personal and domestic purposes. Hence the early peopling of the banks of rivers.

The mouths of rivers were first selected as *harbours* by the neighbouring population, being in some measure *ready-made*, contiguous to the most fertile spots, and sufficiently convenient for all the ordinary purposes of a local and limited trade. But, in an expanded state of maritime enterprize, they exhibit defects of no ordinary magnitude, such indeed as would justify us in considering a river as a nuisance rather than a benefit to a harbour.*

The *bars* of sand or shingle, to which we have referred, produce shallow water at the entrance, and prevent the shipping from passing and repassing, with equal facility, at all times of the tide. The river, too, in the state of flood, passes out to sea with a velocity which few commanders of vessels, for obvious reasons, care to take advantage of even when the current is in their favour; and when the current is in opposition, it prevents vessels from entering the harbour in states of weather when property and life are in jeopardy.

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* The opinion here expressed receives a practical illustration from the harbour of Leith, originally selected, from being the mouth of the water of Leith. Its inconveniences for modern traffic led to the erection of the Newhaven Pier; then the Chain-Pier; and, lastly, to the magnificent harbour of Granton,—excellent, because *without a river*, and destined at no distant period to become the Port of Edinburgh.

In the range of coast which we have at present chiefly in view, viz., from the Moray Frith to the Frith of Forth, there is not a single harbour which can be taken *at all times of tide.* Some have no obstacles, such as bars or river-floods, characters which destroy the value of Aberdeen, Montrose, and Dundee, as harbours of refuge. But, in the absence of these evils, the remaining harbours become dry, or nearly so, at low water; and, consequently, can only be approached towards high water. In such circumstances are the tide-harbours of Peterhead, Stonehaven, Aberbrothick, and St Andrews.

Should a sailing vessel be overtaken by an easterly gale, when off the intermediate part of the coast, between Fifeness and Kinnaird's Head, her situation would be dangerous in the extreme. In but few cases could the tide harbours and those having bars, or under the influence of floods, be approached with any prospect of safety. She must either stand out to sea or bear away, if practicable, for Cromarty Bay or the Frith of Forth. If her course be northward, she has to dread the possibility of being unable to weather Kinnaird's Head, as the *turning point* of the Moray Frith; or if she steer for the south, she has Fifeness as the turning point of the Forth to weather. Should a failure at either of these points take place, very little chance would be left of saving either life or property.*

When we consider the vast amount of shipping, at all seasons of the year, frequenting the coast referred to, and keeping in view, that in its whole extent of upwards of a hundred miles there is not a single harbour of refuge, the expediency of directing public attention to so great a defect, must at once be obvious. Besides, it deserves to be kept in view, that, in this locality, vessels are exposed to " encountering a sea and tide (to use an expression of a committee appointed by the Incorporation of Traders in Leith, relative to the expediency of erecting a light-house on the Bell-Rock), surpassed in few places on the globe."* The truth here stated is too fully corroborated by the shipwrecks which ever and anon are occurring on the portion of the coast referred to, whereby a considerable amount of life and property is annually sacrificed, which the existence of suitable harbours of refuge might greatly reduce.

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* Even steamers are not exempt from the evil above referred to. The London Steamer, which last week should have delivered her goods and passengers at Aberdeen on Tuesday morning (February 21st), was obliged by the easterly gale to seek for shelter in the Frith of Forth, and could not enter her port until Friday morning (February 24th.)

It is true, that the erection of light-houses on the different parts of the coast under consideration, from their sites being judiciously selected, and all their arrangements satisfactorily regulated, has furnished to the shipping an important amount of security. But, in its character, this security is essentially different from that which a harbour of refuge would afford. The former merely enables the mariner to ascertain his position or his danger, the latter receives him into safety. Separately, each has its excellencies, but when conjoined, then only is the maximum of protection furnished to the seaman.

The questions respecting the suitable positions, forms, and materials of the harbours of refuge, cannot, in the present state of our information, be expected to receive a satisfactory reply. We take it for granted that the object in view can only be accomplished by means of a *breakwater*, protecting a bay or convenient portion of the coast from the fury of the waves, and permitting vessels to ride at anchor therein, without strain on their cables, and in comparatively still water. It may also be assumed that the materials for the construction of the breakwater must be *stones.* Logs of timber, it is true, have been proposed as suitable materials for the construction of breakwaters, and their claims on this score have met in some quarters with considerable favour. But although *White's Breakwater*, and all its subsequent modifications, may be advantageously employed *for a few months*, to shelter bathing ground, or protect a fishing-station ; yet the perishable character of the timber, in sea-water, must not be forgotten, when the importence of the permanency and stability of a breakwater are duly considered. The little crustacean, termed *Limnoria terebrans*, which feeds on timber in the sea, and propagates with amazing rapidity, would prove a foe to breakwaters of such materials, and render their maintenance troublesome, precarious, and expensive. *Stones*, therefore, must be employed as the material in the construction of the breakwater; and fortunately blocks of sufficient magnitude and durability are not wanting at various places of the coast.

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* Stevenson's Account of the Bell-Rock Light-house, p. 96.

The best positions for harbours of refuge could be ascertained, with the greatest certainty, by an examination of those mariners who have been accustomed to navigate the coast, and who are, in consequence, familiarly acquainted with the dangerous winds, the *sets* of the tides, and the depths of water.

Towards the turning point of the Moray Frith, a situation occurs in many respects excellent for the formation of a harbour of refuge, viz. Sandford Bay, bounded on the north by Peterhead, and on the south by Buchanness. Here, by means of a breakwater, this Bay, which possesses excellent anchorage ground, with sufficient depth of water, and affords in its present state a great amount of protection against westerly gales, could be made a haven of security equally convenient both for size and proximity to the ship-stores of Peterhead. Materials well adapted for the construction of a breakwater are abundant in the neighbourhood, and the lighthouse on the south side of the bay at Buchanness, would furnish satisfactory directions to the mariner running to it for shelter. There are here no shifting sands to contend against, although an objection may be urged against the locality, as *too near the turning point* into the Moray Frith.

The Bay of Aberdeen offers apparently but few conveniences for the construction of a harbour of refuge. The quantity of shifting sand ranging along the coast from Slains on the north, to Girdleness on the south side, would form obstacles which, probably, no arrangement of walls could prevent from accumulating injuriously.

The Bay of Nigg, immediately to the south of Aberdeen bay, seems to possess several advantages. It is not incommoded with moveable sands, has abundant materials for the construction of the breakwater in the immediate neighbourhood, and possesses a lighthouse on its northern ness. Besides, vessels finding shelter in this locality could readily obtain from Aberdeen a supply of stores, or be taken to the harbour by the steam-tug, to receive the necessary repairs. But it may be added, that the bay itself is rather limited, and the depth of water, perhaps, not altogether suitable for vessels of great draught.

The forms of the bays of Stonehaven, Bervie, and Montrose, do not seem peculiarly adapted for the purpose in view. Lunan Bay, on the other hand, may lay claim to some consideration. In westerly gales, its south side affords anchorage and shelter for small craft; but when the wind is easterly, it is exposed to a heavy sea, and its sandy beach has been the grave of many seamen.

If now we pass over Aberbrothick, which does not hold out any advantages, the coast exhibits nothing but moveable sand onwards to St Andrews. Here a rocky coast commences, extending to Fifeness, and might probably furnish in some spot a site for a harbour. But in such a locality the harbour would be close to the turning point into the Frith of Forth, and might be speedily injured by shifting sands.

There would be little difficulty, even in the absence of a survey executed for the specific object, in making a tolerably close approximation to the best sites for harbours of refuge, if the sea-charts were constructed as they ought to be. But, alas! those in use, at present, are not fitted to convey the requisite information respecting the depth of water, or the prevailing currents, and can scarcely be considered adequate for the ordinary purposes of navigation; nor have we a near prospect of getting our condition bettered. True it is, that suitable materials for the purpose are known to exist; but these are withheld from the public, and will probably continue to be so, unless the public voice demands their production. A Government survey of the east coast of Scotland has been in progress during the extended period of the last score of years. This survey is understood to have been completed northwards to the Pentland Frith. The instruments furnished have been of the best construction, and entrusted to individuals qualified to use them with success; and I have been informed by competent judges, that the observations and drawings which have been produced, possess uncommon merit. Yet have the Lords Commissioners of the Admiralty hitherto kept the produce of so much expense and labour in their repositories, regardless alike of the interests of the shipowners and of science. Like other public boards, in the absence of a little pressure from without on the subject, they have become inactive; while a share of the reproach ought probably to attach to the corporations of the shipping ports of the east of Scotland, who have witnessed the survey proceeding, and have failed to enquire after the results. Let the magistrates of the burghs and sea-ports interested, bestir themselves, and accurate trustworthy charts would soon be accessible to the mariner, an additional protection furnished to life and property, and the limits of physical geography greatly extended.

Having referred to the inactivity of the Lords Commissioners of the Admiralty, in not providing accurate charts for the east coast of Scotland, even after excellent materials have been procured, I shall close this communication by a few remarks on the "time of high water on the full and change of the moon," at different places on the said coast, as given in the Nautical Almanack for the year 1843, p. 556.

As the Zetland Islands are in some degree without the limits to which the preceding remarks apply, we shall merely observe that the time of high water at Scalloway (introduced into the Almanack for the first time in 1841) is made to agree with Balta in Unst, nearly thirty miles to the north of it, both being marked $9^{h}\ 45^{m}$. When the direction of the flood-tide is considered, the more westerly position of Scalloway will not explain the coincidence in apparent time. But how shall we account for the entries relating to "Brassa Sound," and "Lerwick harbour," the former having its high water assigned at 10^{h} the latter $10^{h}\ 30^{m}$! How few who have paid any attention to the harbours of the coast, are ignorant that "Brassa Sound" is "Lerwick Harbour," and that the two names denominate the same commodious haven!

In approaching nearer the scene to which our remarks have a more immediate reference, the "Orkney Isles" have a place in the tide-table, the time of high water being 10^h^ 30^m^ and then, strange to observe, "Cairston" and "Stromness" in the "Orkneys" have each their time of tide set down at 9^h^.

The time of high water of the "Pentland Frith" is stated at 10^h^ 30^m^, while "Duncansby Head," the prominent easterly shoulder of the Frith, has its time of high water set down at 8^h^ 15^m^.

Passing southwards we find "Peterhead" inserted for the first time in the Almanack in 1839, having its time of high water 0^h^ 45^m^, while "Buchanness" is recorded, as of old, at 12^m^. A difference of 45^m^ in the time of tide between two places not a couple of miles apart, and the one situate farthest to the north, whence the flood-tide proceeds, receiving it later, may well excite some degree of surprise.

The port of Aberdeen has evidently attracted considerable notice. In 1839 the time of high water was changed from its ancient period of 0^h^ 45^m^ to 1^h^ 12^m^, and in 1841 reduced by 1^m^, and now appears as 1^h^ 11^m^.

Proceeding southwards along the coast, we find by the table the time of high water stated as the same for "Montrose" and "Tay Bar," viz. 1^h^ 45^m^. The distance between the two places, in the direction of the flood-tide, being about eighteen miles, and the latter being, in time, behind the former less than one minute, we should have here, on the supposition that the entries in the table are correct, a velocity of tidal wave at this part of the coast, greater than any known tidal velocity on the globe, and about thirty-six times greater than its ordinary velocity in the German Ocean in the neighbourhood, which is stated by good authority to be about thirty miles an hour, although there is an authority which fixes its rate at sixty miles.

High water at "Dundee" is stated at 2^h^ 22^m^, or 37^m^ later than "Tay Bar." Taking into account the westerly position of Dundee, the difference will be nearly 38^m^. If we consider the distance between the two places as little more than a dozen of miles, we shall here have an example of *retardation*, compared with the former acceleration of the tidal-wave, of truly unlooked for extent, even keeping in view the influence of depth of bottom.

High water at "Leith" is likewise stated as 37^m^ later than at "Tay Bar;" now the total difference in time from the position of Leith would not reach 39^m, while the distance is about forty miles. This would make the velocity of the tidal-wave from "Tay Bar" to Leith, compared with its velocity from "Tay Bar" to "Dundee," nearly as three to one, and the former more than double its ordinary velocity in the German Ocean.

As all the *Establishments of the Ports*, in the table, are set down to *apparent* time, and the actual times of high water when the moon passes the meridian *at the same time as the sun*, it is probable that, in the reduction, errors may have been introduced rather than corrected, from the state of the data, and that the angular distance of the moon from the sun at the times of observation, may have been overlooked. But some of the anomalies which have been pointed out, in all probability arise from the different standards employed for determining *high water*, known to be in use. Thus we have the time of high water marked by one observer, when the tide-wave has reached its highest *elevation*, by another when *slack* tide occurs, and by a third when the reverse *current* begins to prevail. There is nothing, however, in the table to indicate the employment of a common standard. In illustration of the influence which a variable standard may exercise on the *time* of tide, I may refer to that excellent hydrographer, Mackenzie, who, in reference to the Pentland Frith, says, "On the shore of Swona, it flows till half-past nine on the east side, and till ten on the west side, on the days of new and full moon. In the middle of the Pentland Frith it is *still* or slack water, on the change days at half-past eleven, but the tide does not *turn* till twelve."

Now, whatever be the cause of the anomalies thus apparent in the tide-table of the Nautical Almanack, it is surely of importance, for the credit of such a national work, that the entries which it contains should be accurate and intelligible, or that no tide-table of doubtful character should have a place there.

I should be sorry if these remarks on the charts and tide-table of the east coast of Scotland, even although not remotely connected with the object in view, led the mind of the reader away from contemplating the necessity of establishing

HARBOURS OF REFUGE. This is, indeed, a subject which the shipping interest, and the friends of humanity, are equally bound to bring under the notice and favourable consideration of the British public, and it will be to me a source of pure enjoyment if the preceding remarks tend in any degree to the accomplishment of an end in so many respects desirable.

JOHN FLEMING.

KING'S COLLEGE, ABERDEEN,
March 2. 1843.

On the Formation of the Diamond. By Dr ALEXANDER PETZHOLDT, of Dresden.

Notwithstanding the great diversity of opinion expressed by authors regarding the mode of formation of the diamond, yet all the different views entertained may be included under two principal divisions, viz., those which suppose that it is the direct product of the action of heat on carbonic acid or carbon, and others which support the idea of its being the result of the slow decomposition of plants. It may not be out of place to give a brief account of the most important of these views, previous to communicating my own observations.

While Leonhard* asks, if we may not believe that the origin of diamonds is to be ascribed to carbonaceous sublimations from the interior of the earth, a question which must, on chemical grounds, be answered decidedly in the negative, because carbon is not in the slightest degree volatile ; Parrot† regards diamonds as products of volcanic action, as the result of the operation of the heat on small fragments of carbon. Parrot was first of all led to this view, by his minute examination of Russian diamonds, in the course of which he came to the opinion, that the only way of explaining certain structural phenomena, such as cracks and flaws in the interior, and a scaly appearance on the external surface, combined with black structureless included portions of matter supposed to be carbon, was to assume that a strong red heat had fused the carbon, and that, in consequence of subsequent rapid cooling, the cracks in the interior, and, owing to the separation of individual pieces from the outer surface, the scaly structure, were produced.* The black masses recognisable in the interior are consequently imperfectly fused, condensed, or crystallized carbon. Now, although it cannot be denied that, as regards the Russian diamonds, there is some probability for the supposed mode of formation, because geognostical investigations have proved the vicinity of dolomite, a rock whose origin is generally believed to be connected with volcanic action, and have shewn the probability of the diamonds having been transported by water, from their original matrix in that substance, to their present situation, not to take into consideration the circumstance that, according to my own investigations, no well-founded objection can be made to the possibility of a fusion (softening, liquefaction) of vegetable carbon under certain circumstances; yet, nevertheless, much may be urged in opposition to Parrot's view. First of all, no signs of volcanic activity are to be met with in the diamond districts of other countries, although, in the diamonds produced by them, the same cracks, flaws, and other peculiarities of structure are equally observable ; hence, a different mode of origin must, at all events, be assigned to the non-Russian diamonds. Secondly, no diamonds have been found actually embedded in the dolomite of the Adolphskoï valley. Thirdly, the presence of internal flaws and cracks, and of the scaly structure of the exterior, by no means necessarily involves the assumption of great heat and subsequent rapid cooling in the formation of diamonds ; and we may more naturally ascribe the cracks, &c., to the blows received during the transport of so hard and brittle a substance as diamond, and the external scaling off is solely owing to imperfect crystallization, for the instances of it I have seen have always been in the modified crystalline forms of the diamond (to which all the Russian specimens examined by Parrot belong), and never in the simple octahedrons.

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* Leonhard's *Populärische Vorlesungen*, vol. iii. p. 498.

† Parrot's *Notice sur les diamans de l'Oural*, in the *Mémoires de l'Academie Impériale des Sciences de St Pétersbourg. Série dixième. Sciences Mathematiques*, tom. i., p. 32. He says, "Diamonds are the products of volcanic action exercised on small portions of carbon, or on a substance composed of much carbon and very little hydrogen." See also Leonhard's *Jahrbuch*, 1838, p. 541, where portions of Parrot's Memoir are published.

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* See *Petzholdt's Erdkunde (Geologie).* Leipsic, 1840, p. 189 ; *Petzholdt, de Calamitis et Lithanthracibus. Dresdae et Lipsiae,* 1841, p. 31 ; and *Petzholdt, über Kalamiten und Steinkohlenbildung.* Dresden and Leipsic, 1841, p. 27.

Göbel's[*] view of the origin of the diamond is, it is true, supported by chemistry, in so far that carbon can be obtained from carbonic acid at a high temperature, by means of the action of reducing substances, such as magnesium, calcium, aluminium, silicium, or iron, and a direct experiment of mine regarding the power of iron to reduce carbonic acid is also in its favour;[†] but the geognostical relations in which diamonds are found, by no means confirm this opinion; for we either find no phenomena whatever connected with the occurrence of the diamond, which indicate so high a temperature as would be requisite for the decomposition of carbonic acid, or where such present themselves, as in the case of the dolomite of the Ural, diamonds have not actually been found in the rock. We have not taken the fact into consideration, that when carbon is separated from its combinations, as from carbonic acid, it is always obtained in the form of a black powder.[‡]

Lastly, the opinion expressed by Hausmann[§] must not be passed over in silence, as it is the view entertained by so competent a judge. According to him, electricity has operated in the formation of diamonds, and that by lightning decomposing carbonic acid; and the argument for this is, that, according to the assertion of the oldest diamond seekers, fulgurites or lightning tubes are most frequently met with where the diamonds are most numerous. Though we should assent to the possibility of such a decomposition under certain circumstances, yet we cannot regard as at all admissible, the formation of the crystal from the separated carbon during the short continuance of the electrical action of lightning. The formation of a crystal undoubtedly requires infinitely more time than could be afforded during a flash of lightning, and there is not a single instance known of a body crystallizing suddenly during the continuance of an electric spark.

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[*] Englehardt's *Lagerstätte der Diamanten*, &c.; the chemical portion of that essay was edited by Göbel, and an extract from it is published in Poggendorff's *Annalen*, 1830, vol. xx., p. 539.

[†] See Petzholdt's *Erdkunde* (*Geologie*), p. 133.

[‡] See *Erdmann's Chemie*, 1840, p. 133.

[§] Ersch and Gruber, *Allgemeine Encyclopädie*, article "*Diamant.*"

With regard to the series of opinions according to which the diamond is of vegetable origin, it seems proper to place at their head that of Newton, because, so far as I am aware, it is the oldest, and is at the same time extremely acute. From the great refractive power of the diamond, he concluded it to be a coagulated fatty or unctuous body,* and this idea was started at a time when nothing was known of the chemical constitution, or as to the combustibility of the diamond. This, then, was the first hint of its vegetable origin. Jameson† spoke more decidedly on the vegetable origin of the diamond; for he expressed the opinion, that it must have been separated, as a form of pure carbon, from the sap of some plant, just as silica, in the form of tabasheer, is deposited in the joints of the bamboo and other plants. He adduced, as another proof of his opinion, the remarkable hardness of some woods, as, for example, the *Metrosideros vera* and others, which he ascribed to carbon approaching the condition of the diamond. Lastly, Brewster adhered to the hypothesis of the vegetable origin of the diamond, and thought he was enabled to conclude, from its polarising properties,‡ that it must at one period have been in a soft or pasty condition, but in no degree a product of fire. He further asserted that the former softness of the diamond must have approached most nearly that of hardened gum, and that, like amber, the diamond must have had its origin in the vegetable kingdom, and been the result of decomposition. The crystalline structure of diamonds does not militate against this conclusion; for honeystone is regularly crystallized, although it is undoubtedly of vegetable derivation, as is proved not only by its chemical composition, but also by its mode of occurrence.

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* Murray's Memoir on the Diamond, p. 13; and Froriep's *Notizen*, vol. xvi. No. 22, March 1827.

† Jameson's Speculations in regard to the Formation of Opal, Woodstone, and Diamond, in the Memoirs of the Wernerian Society of Edinburgh, vol. iv. p. 556, and translated in Froriep's *Notizen*, vol. xvi. No. 22.

‡ Quarterly Journal of Science, Oct. 1820. Froriep's *Notizen*, vol. xvi. No. 22. Philosophical Magazine, 3d Series, vol. vii. p. 249. Poggendorff, vol. xxxvi. p. 564. Leonhard's *Jahrbuch der Mineralogie*, 1834, p. 225.

Lastly, we now arrive at our own view of the formation of the diamond, and it coincides completely with that of Newton, Jameson, and Brewster; but we base it neither on its strong refractive power, nor on the great hardness which the carbon has acquired in the diamond, nor on its polarising properties, for we are supported by entirely different considerations. We believe that, according to the present state of our knowledge, the diamond is a product of the newest geological period, resulting from the slow decomposition of a vegetable substance. Let us now shortly adduce the proofs of this opinion.

That the diamond must be a product of the youngest geological epoch, of the so-called historical epoch* in a geological sense, appears from the fact, that hitherto it has only been met with in stony deposits, which decidedly belong to the youngest formations, as I have more fully stated in another place. Its primary repositories, that is to say the places where it was formed, cannot be very different nor very remote from its secondary repositories, that is, from those places where we now meet with it; and all the mineral bodies which we are in the habit of regarding as the more or less constant associates of the diamond in diamond sands, are merely accidental, if I may so express myself. There is not the slightest reason for assuming that the formation of the gold or platina, &c., stands in any nearer connection with the diamond, for platina and gold are found in many localities without diamonds. These bodies were either at the locality when the diamond was formed, or they were transported along with that substance by water. And although it cannot be denied in regard to some of the other ingredients of the diamond-sand, such as some of the minerals belonging to the quartz genus, viz., quartz, calcedony, and hornstone, and also brown ironstone, that they were formed contemporaneously (in a geological sense) with the diamond; yet this circumstance by no means tends to support the idea of any sort of connection between their formation and that of the diamond, because the recent formation of these bodies can be observed every where, and where no diamonds are to be met with. The association of all these substances, which we have termed accidental, is merely caused by the geognostical constitution of the district through which the river-course of the present day extends, by the nature of that course itself, by specific gravity, and by many other circumstances having not the smallest concern with the formation of the diamond. The strongest proof, however, of the recent origin of the diamond, is its occurrence in the loose rolled matter in which and with which it was formed, combined with the want of success that has hitherto attended the search for the diamond embedded in those rocks, regarding which it is so easy, on the other hand, to prove that from them all the other rolled bodies had their origin. We leave entirely aside the question, whether the prevalent popular belief in the East Indies and Brazil, that diamonds are still produced,[*] be an instinctive perception of the truth, or a deceptive notion.

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* Petzholdt's Erdkunde (*Geologie*), p. 87.

Further, the diamond must have been formed in the moist way from a liquid, because otherwise it would have presented none of the included splinters of quartz of which I have spoken in another place,[†] and of which some even exhibit the vegetable cellular texture.

Lastly, from all that we know, the material from which the diamond was formed, by the separation of crystalline carbon, could only have been a substance rich in carbon and hydrogen, such as, owing to the requisite chemical properties, can only be looked for in the vegetable kingdom; and we are forced to consider the diamond as produced from this substance, consisting of carbon and hydrogen, by means of decomposition. The determination of the nature of this process is solely a chemical matter; and Liebig, who has undeniably rendered the greatest service to our knowledge of the decomposition of organic bodies, makes the following remarks :[*]—"If we suppose decay to proceed in a liquid, which contains both carbon and hydrogen, then a compound containing still more carbon must be formed, in a manner similar to the production of the crystalline colourless naphthalin, from a gaseous compound of carbon and hydrogen. And if the compound thus formed were itself to undergo further decay, the final result must be the separation of carbon in a crystalline form. Science can point to no process capable of accounting for the origin and formation of diamonds, except the process of decay. Diamonds cannot be produced by the action of fire, for a high temperature, and the presence of oxygen gas, would call into play their combustibility. But there is the greatest reason to believe that they are formed in the humid way, that is, in a liquid; and the process of decay is the only cause to which their formation can with probability be ascribed."

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[*] See *Leonhard's Populäre Vorlesungen über Geologie*, vol. iii. p. 497.

[†] *Vide* Jameson's Journal for January 1843, p. 187.

As yet we are ignorant of the nature of the vegetable substance, rich in carburetted hydrogen, by whose decomposition the diamond was formed, and as to what were the particular conditions necessary for the appearance of crystalline carbon. This only we know, however, that the whole process was an extremely slow one, and that it could not in any way be hastened by an increased temperature, for in that case the carbon could not have crystallized, but must, on the contrary, have been separated in the form of a black powder.

The conclusion deduced by Newton from certain optical properties of the diamond, viz., that it has been produced from an oily body, is very beautifully confirmed by the newest and most accurate investigations of chemistry, for, according to them, the so-termed oily bodies are proved to be the richest in carburetted hydrogen; and chemistry, which can alone explain the decompositions of bodies, and their formation from their elements, just requires for the formation of the diamond the decomposition of a substance rich in carburetted hydrogen. There are two different phenomena connected with the above explanation of the origin of the diamond, which cannot be left unnoticed, as they are well calculated to place the truth of our assertions in a clearer point of view. As I have already stated elsewhere, diamonds not unfrequently exhibit at their surface blackish spots, which disappear on the application of heat ;* and, moreover, they very frequently present in their interior perfectly black, amorphous bodies, which cannot be considered as any thing else but uncrystallized carbon,—a fact observed in the course of Parrot's investigations, as well as my own. This phenomenon can only be explained by assuming a somewhat accelerated decay of the matter containing carbon and hydrogen ; in the course of which the carbon has been produced in the form of a black powder, instead of being separated in a crystalline state. On the other hand, I have on several occasions had an opportunity of convincing myself of the tendency of carbon to crystallize, when the combustion (the accelerated decay†) of a substance rich in carbon and hydrogen is retarded. Thus, on the wicks of badly burning tallow candles, I have seen the well-known accumulations of carbonaceous matter (soot), which have generally globular or semi-globular forms, assume distinctly an octahedral shape ; and I believe that this appearance has long been observed by others, for it is only by the resemblance of an octahedron to the envelope of a letter that I can explain the popular saying, of there being a letter in the wick of a candle. I have even preserved, for some time, one of these tolerably well-defined octahedrons, and exhibited it to my class ; but it was at last broken, and it then appeared that the fragments were harder than the ordinary soot, although they could still be easily bruised between the fingers.

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[*] *Liebig's Organische Chemie in ihrer Anwendung auf Agricultur und Physiologie. Braunschweig*, 1840, p. 285; and Playfair's Translation, p. 143.

Lastly, let me add a few words regarding the experiments made in recent times on the production of artificial diamonds, for I believe that I may say, without exaggeration, that, since it was discovered that the diamond consists of pure carbon, there is hardly any chemist who has not performed more or less extensive experiments on the subject. That the results of such investigations have been published by but few chemists, is no proof that few experiments have been made, for human nature and vanity prefer silence to publicity, where investigations have failed, and hopes have been disappointed.

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* See Parrot, *Notice sur les Diamans*, p. 30 and 31.

† That combustion is only a rapid decay, and decay only a slow combustion, is known to all chemists. Above all, see Liebig's remarks on this subject in the second part of his Organic Chemistry.

All the experiments to form artificial diamonds may be referred to two methods, viz. the attempt to fuse carbon, and the endeavour to separate carbon in a crystalline state from a highly carbonaceous compound, by means of decomposition. It need hardly be remarked that all the trials have hitherto been in vain. The experiments made with the first view have been rendered unsuccessful by the infusibility of carbon, and the others proceeding on the second idea have always resulted in the production of carbon in the form of a black substance.* Lastly, if any one should be of opinion that, by the assistance of a constantly operating electrical stream, highly carbonaceous bodies might be decomposed so slowly that carbon might be separated in a crystalline condition, that is, in the form of diamond, just as copper and the other metals have been recently obtained, in a crystalline state, from solutions, by Jacobi's method, such an expectation will prove to be a vain one; for, on the one hand, the substances most suited to galvanic decomposition are non-conductors of electricity, as, for example, sulphuret of carbon, oil of turpentine, copaiva balsam, &c.; and on the other, if we should be successful in separating, from any compound, crystalline carbon on the conducting wire, yet, according to theory, at the very moment when even the most delicate covering of crystalline carbon should be deposited, all further action on the decomposing liquid would be interrupted, for the matter of diamond itself is known to be a non-conductor of electricity.†

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* A pretty extensive collection of the experiments on this subject, together with the references, is to be found in Ersch and Gruber's *Allgemeine Encyclopädie der Künste und Wissenchaften*, under the article *Diamant*. See also in *Gmelin's Handbuch der Theoretischen Chemie*, vol. i. the chapter on Carbon.

† From Petzholdt's *Beiträge zur Naturgeschichte des Diamantes*, 1842,

An Attempt to determine the mean height of Continents. By Baron Von HUMBOLDT.

AT the meeting of the Berlin Academy of Sciences, on 18th July 1842, a memoir by M. de Humboldt was read, of which we think it necessary to give a somewhat lengthened account. It is entitled " An attempt at determining the mean height of Continents."

" Among the numerical elements on which the progress of physical geography appears more particularly to depend, there is one which no attempt has been hitherto made to determine. The notion which seemed to prevail, that it was impossible to come to such a determination, has perhaps been the principal cause of the subject being neglected. However, the extension of our orographical knowledge, as well as the greater accuracy of the maps which represent large portions of country, determined me, says M. de Humboldt, to undertake, some years ago, a work of great labour, and in appearance barren in results, the object of which is the knowledge of the mean height of continents, and the determination of the mean height of the *centre of gravity of their volume*. In such a case as this, as with many others, such as the dimensions of the globe, the probable distance of the fixed stars, the mean temperature of the poles of the earth, the thickness of the atmospheric stratum above the level of the sea, or the enumeration of the general population of the globe, we arrive at *limited numbers,* between which the results must fall. In like manner, it is by the perfect knowledge of the geometrical and hypsometrical surface of a country, of France, for example, that we may thus be led, by analogy, to extend the conclusions to a great part of Europe and America, and are enabled to establish numerical data, which have recently been completed in a very satisfactory manner in regard to central and western Asia.

" It was likewise necessary to collect, with the greatest care, astronomical determinations of the height of places, in order to establish, to about 300 or 400 metres of absolute height, the limits between the acclivities of the mountains and the edges of the valleys. I long since demonstrated the possibility of such a determination of limits, and, from the comparison which depends on it, I have deduced the extent of the surface of the plains, and the horizontal and flat portions of mountains, in my geognostical researches on South America; a portion of the globe in regard to which the length of the immense wall which forms the Cordillera of the Andes, and of the elevated masses of Parima and Brazil, was so incorrectly limited and circumscribed on all maps. In fact, there is a general tendency in all graphic representations to give the mountains a greater degree of breadth than they really possess, and even in the flat portions to confound plateaux of various kinds with each other."

M. de Humboldt published, in 1825, two memoirs inserted in the Memoires dê l'Académie des Sciences of Paris, on the mean height of continents, and an estimate of the volume of the elevated ridges of mountains, compared with the extent of the surface of the lower regions. An assertion of Laplace in the *Mécanique Céleste* (vol. v., book xi. chap. i. page 13), gave rise to these researches. This great geometer had established in principle, that the agreement observed between the results of experiments made with the pendulum and the compression of the earth, deduced as well from the trigonometrical measurement of the degrees of the meridian as from the inequality of the moon, furnished a proof " that the surface of the terrestrial spheroid would be nearly that of equilibrium, if that surface became fluid. Hence, and from the consideration that the sea leaves vast continents uncovered, we conclude that it cannot be of great depth, and that its mean depth is of the same order as the mean height of the continents and islands above its level, a height which does not exceed 1000 metres" (or 3073 Parisian feet, that is to say, only 463 feet less than the summit of the Brocken, according to M. Gauss, or a little more than the most elevated mountains of Thuringia). Laplace further adds, " This height is, then, a small fraction of the excess of the radius of the equator over that of the pole, an excess which exceeds 20,000 mètres. Just as high mountains cover some parts of continents, so there may be great cavities in the bed of the sea; but it is natural to suppose that their depth is less than the elevation of high mountains, as the deposits from the waves, and the remains of marine animals, must have tended, in the lapse of time, to fill up these great cavities."

Considering the profound and extensive knowledge which the author of the *Mécanique Céleste* possessed in the highest degree, an assertion of this nature was the more striking, as he could not be ignorant that the most elevated plateau of France, that from which the extinct volcanoes of Auvergne have risen, does not rise, according to Ramond, to more than 1044 feet, and that the great Iberian plateau is not, according to my own measurements, more than 2100 feet above the level of the sea. Laplace has therefore fixed the upper limit at 1000 metres, merely because he has considered the extent and the mass of the elevations of mountains to be much greater than they really are, inasmuch as he has confounded the height of the insulated peaks or culminating points with the mean height of the mountain ridges; he has admitted much too low a number for the depth of seas, because, in his time, data could not be found on the subject, and he has thence inferred the proportion of the extent of the surface (in square miles) in regard to all continents, to the extent of the projection of the surfaces covered by mountains.

A very exact calculation has shewn that the mass of the chain of the Andes, in South America, from where it leaves the whole portion of the eastern plains of the pampas and forests, regions whose surface is one-third larger than that of Europe, does not rise above 486 feet. M. de Humboldt hence concludes, "That the mean height of continental lands depends much less on those chains or longitudinal ridges of little breadth which traverse continents, and on their culminating points or domes, which attract common observation, than on the general configuration of the different orders of plateaux and their ascending series, and on those gently undulating plains with alternating slopes, which have an influence, by their mass and extent, on the position of a mean surface, that is to say, on the height of a plain placed in such a manner that the sum of its positive ordinates shall be equal to the sum of its negative ordinates."

The comparison which Laplace has instituted in the passage quoted from the *Mécanique Céleste* between the depth of the sea and the height of continents, recalls a passage of Plutarch, in the 15th chapter of his Life of Æmilius Paulus (ed. Reiskii, vol. ii. page 276),—a passage the more remarkable, as it makes us acquainted with an opinion which generally prevailed among the philosophers of the Alexandrian school. After quoting an inscription found on Mount Olympus, and giving the result of the measurement of its height by Xenagoras, Plutarch adds, "But geometricians (probably those of Alexandria) believe that *there is no mountain higher, and no sea deeper, than ten stadia*." We can entertain no doubt about the exactness of the measurement made by Xenagoras; but it is striking to observe, that the philosophers of this school established in the structure of the earth a perfect equality between the heights or positive and negative ordinates. Here the maximum of the heights and depths is alone taken into account, and not the mean height,—a consideration which rarely presented itself to the mind of the ancient philosophers, and which, for variable magnitudes, was applied in a useful manner to astronomy by the Arabs. Even in the *Metereologius* of Cleomedes (i. 10), we meet with an assertion similar to that of Plutarch; while in the *Meteorologicis* of the philosopher of Stagira (Arist. Met. ii. 2), the only point considered is the influence of the inclination of the bottom of the sea, from east to west, on its currents.

When we try to determine the mean height of the elevation of continents above the present level of the seas, it means that the object is to find the centre of gravity of the volume of these continents above that level,—an investigation very different from that which consists in searching for the centre of gravity of the volume of the continental mass, or the centre of gravity of the masses, seeing that the portion which rises above the sea, in the crust of the globe, is by no means of the same density, as has been demonstrated both by geognosy and experiments with the pendulum. The mode of simple calculation is as follows:—Each chain of mountains is considered as a triangular prism placed horizontally. The mean height of the defiles or passes, which determine the mean height of the crest of the mountains, is the height of the ridge of the prism vertically above the surface, which constitutes the base of the chain. The plateaux are calculated as straight prisms, in order to establish their solidity.

For the purpose of giving an example, taken from Europe, of this kind of calculation, M. de Humboldt states, that the surface of France contains 10,087 square geographical miles. According to M. Charpentier, the Pyrenees cover 430 of these square miles; and, although the mean height of the summits of the Pyrenees rises to 7500 feet, M. de Humboldt makes a reduction upon it, on account of the erosions produced on the prism supposed to be lying horizontally, and which have tended specially to diminish the size of the deep transverse valleys. The effect of the Pyrenees on the whole of France is not more than 35 metres or 108 feet; that is to say, it is to that extent that the normal surface of the entire plain of France would be increased, and the elevation of that surface by the comparison of a great number of very accurate measurements at places towards the centre (such as Bourges, Chartres, Nevers, Tours, &c.) has been found to be 480 feet. This calculation, which M. de Humboldt has made along with M. Elie de Beaumont, furnishes the following general result, in measures thus given by the author:—

	Toises.
1. Effect of the Pyrenees,	18
2. The French Alps, the Jura, and the Vosges, a few toises more than the Pyrenees; common effect,	20
3. The plateaux of Limousin, Auvergne, the Cevennes, Aveyron, Forez, Morvant, Cote d'Or; common effect, nearly equal to that of the Pyrenees,	18
Now, as the normal height of the plain of France is at its maximum about	80
It follows that the mean height of France does not exceed	136 toises, or 816 feet.

The Baltic, Sarmatian, and Russian plains are separated from those of the north of Asia only by the meridian chain of the Oural. It is for this reason that Herodotus, who was acquainted with the connection of the southern extremity of the Oural in the country of the Issidones, called the whole of Europe to the north of the Altai Mountains, Asia. In the neighbouring region of the Baltic plains, near the shores of the Baltic Sea, there are partial elevated masses which deserve particular attention. To the west of Dantzic, between that town and Butow, at the point where the shore of the sea advances much to the north, there are many villages situated at a height of 400 feet; the Thurmberg, moreover, the measurement of which has given rise to many hypsometrical controversies, rises, according to the trigonometrical observations of Major Baeyer, to 1024 feet, which is perhaps the greatest elevation to be found between the Harz and Oural. It is surprising that, according to the measurements made by M. Struve of the culminating point of Livonia, the Munamaggi, this mountain rises only 4 toises higher than the Thurmberg of Pomerania; while, on the other hand, according to Captain Albrecht's chart, the greatest depth of the Baltic Sea, between Gothland and Windau, is not more than 167 toises, a measurement almost identical with that of the Thurmberg.

The flat countries exclusively European, the normal height of which cannot be estimated at more than 60 toises, occupy, according to exact measurements, a surface nine times that of France. The extraordinary extent of this low region is the cause of the mean continental height of all Europe, over an extent of 17,000 square geographical miles, being 30 toises below the result we have found for France. As to the rest, not to occupy more time with numbers, M. de Humboldt adds, that an important consideration in the study of the general phenomena of geology is, that the elevated masses, over extensive countries, in the form of plateaux, produce an entirely different effect on the elevation of the centre of gravity of the volume from that of chains of mountains, when they have the same importance in breadth and in height. While the Pyrenees produce scarcely the effect of a single toise on the whole of Europe, the system of the Alps, which cover a surface almost quadruple that of the Pyrenees, has the effect of 3½ toises; the Iberian peninsula, with its compact massive plateau of 300 toises, produces the effect of 12 toises. The plateau just named, therefore, has an effect on the whole of

Europe four times more considerable than the system of the Alps. This result of calculations is the more satisfactory as it appears to be deduced without reference to any previous hypothesis.

We have recently acquired many new ideas respecting the configuration of Asia. The effect of the elevated colossal masses of the southern portion is found to be weakened, since one-third of the whole continent of Asia, a portion of Siberia, which alone exceeds by a third the entire surface of Europe, does not reach a normal height of 40 toises. This is, likewise, the height of Orenbourg, on the northern shore of the Caspian Sea. Tobolsk does not attain the half of this height, and Casan, which is five times more distant from the shore of the Icy Sea than Berlin is from the Baltic, is scarcely half the height of the last mentioned city. In Upper Irtysch, between Buktormensy and Lake Saysan, at a point nearer the Indian than the Icy Ocean, M. de Humboldt has found that the plains only reached a height of about 800 feet; this, however, has been called the plateau of Central Asia, and is not half the height of the streets of the city of Munich above the sea-level. The celebrated plateau between Lake Baikal and the Wall of China (the stony desert of Gobi and Cha-mo), which the Russian academicians, MM. Bunge and Fuss, have measured with the barometer, has a mean height of only 660 toises, which is nearly the same as that of the Müggelsberg at the summit of the Brocken. There is, moreover, in the centre of this plateau, at the point where Ergi is situated (lat. 45° 31′) a cauldron-shaped depression, the bottom of which descends to 400 toises, that is to say, the height of Madrid. "This depression," says M. Bunge, in a memoir not yet published, "is covered with Halophytes and species of the genus *Arundo*, and, according to the tradition of the Mongolians who accompanied us, it was formerly a great inland sea." The two extremities of this ancient inland sea are bounded by steep rocks, just like an ordinary sea, in the neighbourhood of Olonbaischan and Zukeldakan.

The surface of Gobi, in its masses of uniform elevation, and from the south-west to north-west, is twice as large as that of all Germany, and will raise the centre of gravity of Asia

20 toises; while the Himalaya and the Houen-lun, which is a prolongation of the Hindoo-Kho, with the plateaux of Thibet, which connect the Himalaya with the Kouen-lun, will only produce an effect of 56 toises. In the examination of the considerable relief between the plains of the Indus and the depressed plateau of Tarim, which, on leaving Kaschgar, inclines to the east towards Lake Lop, it is necessary to examine with more care the point near the meridian of Kaylasa, and the two sacred lakes of Manasa and Ravana-Brada, on leaving which the Himalaya no longer runs from east to west parallel with the Kouen-lun, but takes the direction from south-east to north-west, and reunites at the projecting ridges of Tsun-ling. The altitudes of the numerous passes of Bamian, as far as the meridian of Tschamalari (24,400 feet), by which Turner reached the Thibetian plateau of H'Lassa, are likewise known for an extent of 21° of longitude. The greater part of them present a very uniform height of 14,000 English feet, or 2200 toises, a height which is not of rare occurrence in the passes of the chain of the Andes. The great route which M. de Humboldt followed from Quito, on his way to Cuença, was, for example, at Assuay (Ladera de Cadlud), and without snow, of the height of 2428 toises, that is to say, 1400 feet higher than this pass of the Himalaya. The passes, as has been stated, give the mean height of mountains.

In a memoir on the relations between elevated summits or culminating points, and the height of mountain chains, M. de Humboldt has demonstrated that the chain of the Pyrenees, calculated from twenty-three passes, was 50 toises higher than the mean chain of the Alps, although the culminating points of the Pyrenees and the Alps were in the proportion of 1 to 1 $\frac{4}{10}$. As the insulated passes of the Himalaya, for example, the Niti-Gate, by which we penetrate into the plain of the Cashmere goats, rise to the height of 2629 toises, M. de Humboldt has not admitted for the height of the Himalayan chain 14,000 English feet, but he proposes to fix it, although perhaps the elevation may be still too considerable, at 15,500 feet, or 2432 toises. The plateau of the three Thibets of Iscardo, Ladak, and H'Lassa, is a prominence between two chains which unite with each other (the Himalaya and the

Kouen-Lun). Mr Vigne's travels in Baltistan, which have just appeared, the journal of the brothers Gerard, published by Lloyd, as well as the recent investigations undertaken in India respecting the relative height of perpetual snow on the Indian and Thibetian declivities of the Himalaya, have demonstrated that the mean height of the Thibetian plateaux has hitherto been greatly exaggerated. In his work entitled "Central Asia," of which only a few pages of the third volume have been yet printed, and which will be accompanied by a hypsometrical map of Asia from the Phasis, as far as the gulf of Petcheli, and from the common embouchures of the Ob and the Irtysch to the parallel of Delhi, M. de Humboldt thinks that he has demonstrated, by bringing together a multitude of facts, that the prominence between the Himalaya and the Kouen-Lun (chains which form the southern and northern limits of Thibet), does not rise above the mean height of 1800 toises, and that it is, consequently, 200 toises lower than the plateau of Lake Titicaca.

The hypsometrical configuration of the Asiatic continent is perhaps still more remarkable for its plains and depressions, than for its colossal heights. This continent is distinguished by two principal characteristic features; 1st, by the long series of meridian chains, which, with parallel axes, but alternating with each other (having perhaps been projected *comme des filons*) extend from Lake Comorin, opposite Ceylon, to the shores of the Icy Sea, in a uniform direction from south-south-east to north-north-west, under the name of Ghates, the Soliman chain, Paralasa, Bolor, and Oural. This alternating situation of auriferous meridian chains (Vigne has recently visited, on the eastern declivity of Bolos, in the valley of Basha, in Baltistan, the auriferous sands mined, according to the Thibetians, by marmots, and, according to Herodotus, by large ants) reveals to us this law, that none of the meridian chains just named, between 64° and 75° of longitude, extend themselves upon the adjoining ones, either towards the east or the west, and that each of these longitudinal elevations does not begin to shew its extent, until a point is reached where the preceding has completely disappeared. 2d, Another characteristic trait in the configuration of Asia, and which has not been sufficiently observed, is the continuity of a considerable elevation, east and west, between 35° and 36½° of latitude, from Takhialoudag, in ancient Lycia, as far as the Chinese province of Houpih, an elevation thrice intersected by meridian chains (Zagros, in Western Persia, Bolos, in Affghanistan, and the chain of Assam, in the valley of Dzangho) from the west to the east of this chain, from the parallel of Dicearchus, which is at the same time that of Rhodes, Taurus, Elbrouz, Hindou-Kho, and Kouen-Lun or A-Neoutha. In the third book of the geography of Eratosthenés, we find the first germ of the notion of a chain of mountains (Strabo, xv. p. 689, Cas.) running in a continuous manner, and dividing Asia into two parts. Dicearchus perceived the connection between the Taurus of Asia Minor and the snow-covered mountains of Asia, which had acquired so much celebrity among the Greeks by the false accounts of those who had accompanied the Macedonians. Importance was assigned to the parallel of Rhodes, and to the direction of this endless chain of mountains. The chlamyde of Asia ought to be found further on under this parallel (Strabo, xi. p. 519), and perhaps, says Strabo, a little more to the east there may be another continent. The Taurus and the plateaux of Asia Minor disclosed for the first time to the Greek philosophers the influence of height on temperature. "Even in the southern latitudes," says the great geographer of Amasis, (Strabo, ii. p. 73) when the climate of the northern coasts of Cappadocia is compared with that of the plains of Argaios, situated 3000 stadia further south, the mountains and all the elevated lands are cold, even when these lands consist of plains." Strabo is the only one among Greek authors who has made use of the word *οϱοπεδια* or mountain plain.

According to the final result of the whole of M. de Humboldt's investigations, the maximum assigned by Laplace for the mean height of continents is too considerable by two-thirds. He found the following numerical elements for the three quarters of the world which have been the object of his calculations (Africa not yet presenting a sufficient number of data to be included).

Europe,	105 toises	(205 metres).
North America,	117 ...	(228 ...).
South America,	177 ...	(345 ...).
Asia,	180 ...	(351 ...).

For the whole of the new continent we have 146 toises (285 metres), and for the height of the centre of gravity of the volume of all the continental masses (Africa excepted) above the level of the present seas, 157.8 toises or 307 metres.

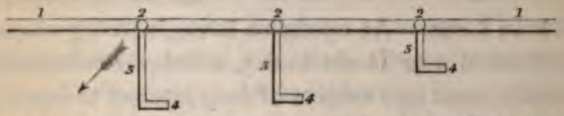
Von Hoff, who has measured with extreme accuracy 1076 different points, the greater part of them in the mountainous portion of Thuringia, over an extent of 224 square geographical miles, estimates that there are about five heights for each square mile, but that these heights are unequally scattered. M. de Humboldt has asked Von Hoff, always for the purpose of verifying Laplace's hypothesis respecting the mass of continents, to calculate the mean height of the hypsometrical measurements which he has made. This philosopher has found it to be 166 toises, that is to say, 8 toises more than the result at which M. de Humboldt had arrived. We ought thence to conclude, that, since a very mountainous country of Thuringia was measured, the number, 157 toises, or 942 feet, is a limit rather too high than too low.

In the certainty in which we now are respecting the progressive and partial rising of Sweden (one of the most important facts in physical geography, for a knowledge of which we are indebted to M. de Buch), we may suppose that the centre of gravity will not always continue the same. At the same time, considering the smallness of the masses which are raised and the weakness of the subterranean forces in action, it may be presumed, regarding such variations, that they will in a great measure compensate each other, and that the position of the centre of gravity above the ocean will not be much changed; but a new circumstance, which appears to result from the numerical calculations of this hypsometrical labour, is, that the smallest heights in our hemisphere belong to the continental masses of the north. Thus Europe has furnished 105 toises, North America 117 toises. The prominent character of Asia between 28° and 40° of latitude compensates the subtractive effect of the lower portions of Siberia. Asia and South America give 180 and 177 toises. We thus read, so to speak, in these numbers, in what portions of the surface of our globe vulcanism, that is to say, the reaction of the interior on the exterior, has been felt with greatest intensity in the ancient *soulèvements*. (*L'Institut*, 5th Jan. 1843 p. 4.)

Notice of the Great Explosion at Dover. Contained in a Letter to the Earl of CATHCART, by Captain STUART, 7th Royal Fusiliers. Communicated by LORD GREENOCK.

DOVER, 26*th January* 1843.

MY DEAR LORD,—An operation in engineering was successfully performed near Dover to-day, which, from its magnitude and novelty, must be a subject of deep interest to every person acquainted in the least degree with practical science. It was the removal of an enormous mass of the cliff facing the sea, which formed an obstruction to the line of railroad. To give you a distinct idea of its position, it may be necessary to inform you, that a portion of the cliff which was penetrated by the tunnel made through Shakspeare's Cliff gave way about two years ago. About fifty yards of the tunnel were carried away, and a clear space was so formed for the line of railroad, with the exception of a projecting point, which, prior to the slip alluded to, was the extremity of the part of the cliff pierced by the tunnel, and to remove which was the object of the operation in question. Mr Cubitt is the engineer, under whose management it took place. The expense of clearing it away by the tedious process of manual labour, would have exceeded L.12,000, and this consideration, as well as the time that would have been lost, induced him to try the bold experiment of blowing it away with gunpowder. It cannot be denied, that there was apparent danger in the undertaking, for the weight of the mass to be removed was estimated at 2,000,000 tons, and the quantity of powder used was more than eight tons, or 18,000 lbs. 12,000 lbs. was the quantity used in blowing up the fortifications of Bhurtpore, and this, I believe, was the greatest explosion that ever (previously) took place for any single specific object. I had several opportunities of seeing the preparations for this grand event. The front of the projection was about 100 yards wide; this front was pierced with a tunnel about six feet in height, and three in breadth; three shafts equidistant from each other and from the entrances to the tunnel, were sunk to the depth of seventeen feet, and galleries were run, one from each shaft, parallel with each other, and at right angles with the line of the tunnel. These galleries varied in length, the longest having been 26 feet, the shortest 12 feet, and, at their extremities, chambers were excavated in a parallel direction with the tunnel. The following rude sketch may give a clearer idea of it.



1. The Tunnel. 2. The Shafts. 3. The Galleries. 4. The Chambers.

In the chambers, the powder was deposited in three nearly equal quantities; it was done up in 50 lb. bags, and the proportion in each chamber was contained in a wooden case nearly as large as the chamber itself. Ignition was communicated by means of a voltaic battery. Conductors 1000 feet in length were passed over the cliff, one to each chamber, and the electric fluid was communicated in a shed built for the purpose on the top of the cliff about fifty yards from the edge. The explosion was conducted by Lieutenant Hutchinson, R. E., who, you may recollect, was engaged under General Paisley, in blowing up the wreck of the Royal George. Two o'clock P.M. of this day, the tide being then at its lowest ebb, was fixed on for the explosion to take place. The arrangements were the best that could be made to preserve order, and as far as possible prevent danger. A space was kept clear by a cordon of the artillery, and the following programme was issued:

Signals, January 26. 1843.

1*st*, Fifteen minutes before firing, all the signal flags will be hoisted.

2*d*, Five minutes before firing, one gun will be fired, and all the flags will be hauled down.

3*d*, One minute before firing, two guns will be fired, and all the flags (except that on the point which is to be blasted) will be hoisted again.

These signals were given exactly at the specified time, and when the expected moment arrived, a deep subterranean sound was heard, a violent commotion was seen at the base of the cliff, and the whole mass slid majestically down, forming an immense debris at the bottom. The success of the undertaking equalled the most sanguine hopes, and exceeded the expectations of all. It was a splendid triumph of skill, and reflects the highest credit on Mr Hutchinson and Mr Cubitt.

Sir John Herschel also gives an account of this *Explosion* in the following letter, addressed to the Editor of the Athenæum:—

Having witnessed the great explosion at Dover, on Thursday the 26th, from the summit of the cliff next adjoining it to the southward, and from the nearest point to which any access was permitted, I would gladly place on record, in your valuable journal, some features of its magnificent operation, which struck me at the time as extremely remarkable, and which have not, I think, been adequately placed before the public in any account that I have seen. These features are, the singular and almost total absence of all those tumultuous and noisy manifestations of power which might naturally be expected to accompany the explosion of so enormous a quantity (19,000 lb.) of gunpowder, and which formed, I have no doubt, the chief attraction of many who came from great distances to witness it,—viz. noise, smoke, earthquake, and fragments hurled to vast distances through the air.

Of the noise accompanying the immediate explosion, I can only describe it as a low murmur, lasting hardly more than half a second, and so faint, that had a companion at my elbow been speaking in an ordinary tone of voice, I doubt not it would have passed unheeded. Nor was the fall of the cliff (nearly 400 feet in height, and of which no less than 400,000 cubic yards were, within an interval of time hardly exceeding ten seconds, distributed over the beach, on an area of 18 acres, covered to an *average* depth of 14 feet, and in many parts from 30 to 50) accompanied with any considerable noise, certainly with none which attracted my own attention, or that of several others similarly stationed, with whom I afterwards compared notes. A pretty fresh breeze from the south-west might be regarded as influential in wafting it away, were it not that the fall took place under the lee of the cliff on whose edge we were stationed.

The entire absence of *smoke* was another and not less remarkable feature of the phenomenon. Much *dust*, indeed, curled out *at the borders* of the vast rolling and undulating mass, which spread itself like a semi-fluid body, thinning out in its progress; but this subsided instantly; and of *true smoke* there was absolutely not a vestige. Every part of the surface was immediately and clearly seen—the *prostrate* flagstaff (speedily re-erected in the place of its fall)*—the broken turf which a few seconds before had been quietly growing at the summit of the cliff, and every other detail of that extensive field of ruin, were seen immediately in all their distinctness. Full in the midst of what appeared the highest part of the expanding mass, while yet in rapid motion, my attention was attracted by a tumultuous and somewhat upward-swelling motion of the earth, whence I fully expected to see burst forth a volume of pitchy smoke, and from which my present impression is, that gas, *purified from carbonaceous matter in passing through innumerable fissures of cold and damp material*, was still in progress of escape; but, whether so or not, the remark made at the moment is sufficient to prove the absence of any impediment to distinct vision.

As regards the amount of tremor perceived, I must confess having speculated with some little anxiety on the probable stability of the abrupt and precipitous ridge on which I stood; and might, therefore, have somewhat underrated the exceedingly trifling movement which actually reached that point, and which I think I have felt surpassed by a heavy waggon passing along a paved street. The impression, slight as it was, was single and brief, and must have originated with the first shock of the powder, and not from the subsequent and prolonged rush of the ruins, which I can positively say communicated no perceptible tremor whatever.

I have not heard of a single scattered fragment, flying out *as a projectile*, in any direction; and altogether the whole phenomenon was totally unlike any thing which, according to ordinary ideas, could have been supposed to arise from the action of gunpowder. Strange as it may seem, this contrast between the actual and the expected effects, gave to the whole scene a character rather of sublime composure than of headlong violence, of graceful ease than of struggling effort. How quietly, in short, the gigantic power employed performed its work may be gathered from the fact, that the operators themselves who discharged the batteries were not aware that they had taken effect, but thought the whole affair a failure, until re-assured by the shout which hailed its success.

The remarkable absence of noise and tremor which characterized this operation is explained by the structure of chalk as a material, and by the rifty state of the cliff as a body. Of all substances, perhaps, chalk is the worst adapted for conveying sound, and the best for deadening the vibration propagated through it by a heavy blow. The initial hammer-like impulse of the newly-created gas on the walls of the chambers of the mines (of which it must be recollected there were *three*, simultaneously exploded) was doubtless thus deadened by traversing at least 75 feet of chalk, even in the shortest direction, or line of least resistance; and *this* must have taken place before the mass could have been sensibly moved from its seat by the expansive force generated, which, however vast, proved incapable (as, indeed, it was expressly provided it should be) to communicate to its enormous load any greater velocity than barely sufficient to rift and bulge it outwards, leaving gravity to do the rest. Nothing can place in a more signal light the exactness of calculation which (basing itself on a remarkably simple rule, the result of long practical experience) could enable the eminent engineer (Mr Cubitt), by whom the whole arrangements are understood to have been made, so completely to task to its utmost every pound of powder employed, as to exhaust its whole effort in useful work—leaving no superfluous power to be wasted in the production of useless uproar or mischievous dispersion, and thus saving at a blow not less than L.7,000 to the railway company.—I have the honour to be, &c. J. F. W. HERSCHEL.

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* It has been stated, that the flagstaff continued erect, but this (if I can credit the distinct evidence of my own senses) is incorrect.

Collingwood, Jan. 31, 1843.

On the Introduction into Scotland of Granite, for Ornamental Purposes, by Messrs Macdonald and Leslie of Aberdeen. By Professor TRAILL, F.R.S.E., M.W.S., &c.* Communicated by the Author.

The first idea of employing the refractory, but enduring, material, granite, in sculpture appears to be due to the ancient Egyptians. Those who have enjoyed opportunities of examining their colossal buildings have acknowledged the precision, and even delicacy, of the figures and ornaments, with which that ingenious people contrived to enrich their architecture. Specimens of their sculpture in granite, which have for 3000 years resisted the action of the elements, and the yet more destructive influence of barbarous invaders, still astonish us by the high polish of their surfaces, and the delicate finish of their details. Even a visit to the Egyptian Saloon of the British Museum, will prove that in accuracy of muscular delineation, and in the communication of absolute *fleshiness* to the lips and features of some of the figures there preserved, the ancient Egyptians evinced a high perfection in the art of sculpture, in a material of the most imperishable kind, on which few succeeding artists have ventured to employ the chisel.

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* Read to the Wernerian Society 18th March 1842.

In our own times, the fabrication of slabs, pedestals, and vases, in hard porphyries, and in granite, has been carried to great perfection in Sweden. The quarries of Blyberg at Elfdalen, for many years, have furnished materials for Swedish ingenuity and skill. The elegant forms and high finish of their works in those refractory materials have contributed greatly to the splendour of the Swedish Capital, and are known and admired over Europe. Yet, though our own mountains yield no less beautiful and durable materials, it is surprising how long we have remained without any attempt to apply them to the purposes of ornamental art. It is true, that, for more than half a century, Aberdeen has exhibited a city chiefly built of blocks of hewn granite; that more lately, this same material has been employed in the construction of Waterloo Bridge in London, and in a few other works; and that Cornish granite appears in the pedestals of a few statues in some of our towns. But the idea of giving a polish, equal to that of ancient Egypt, to our granite in works of considerable size, of introducing this splendid material as a domestic ornament in our halls and saloons, and as lasting memorials of departed worth in our cemeteries, is undoubtedly due to two citizens of Aberdeen, Messrs MACDONALD and LESLIE, who carry on extensive works in that town; where the grey granite of Aberdeen, and the rich red granite of Peterhead, are cut into an endless variety of ornamental articles, which receive the highest polish.

A late visit to their establishment convinced me, that these gentlemen have reduced to practice the difficult problem of giving any required form to so stubborn a material as granite, and of communicating to its surface an exquisite polish, which shew it to be well suited for domestic ornament, and as a superb decoration for the abodes of rank and opulence. The rich warm tint of the Peterhead granite, in particular, will harmonize better with the gilded ornaments and gorgeous hangings of a modern gallery or superb saloon, either as tables or as pedestals for works of art, than furniture made of the most costly woods, or even than the snowy marble of Carrara.

For monumental work, this enduring material possesses advantages over the best marble. In our climate, the effects of rain, sudden frosts, and succeeding thaws are soon perceptible on Carrara marble, or any other kind exposed freely to the weather. Marble thus soon loses its glossy surface, it contracts greenish stains from the vegetation of minute *Byssi*, and inscriptions, in a few years, from these causes, become illegible. The polished granite of Aberdeenshire retains its polish most perfectly under all atmospheric changes, does not contract any stain from vegetation; and, unless wantonly mutilated, will transmit the inscription engraven on it to distant ages. The sharpness of the Egyptian hieroglyphics, carved in a very similar rock 3000 years ago, at this day, proves the durability of granite carving. A beautiful cenotaph of red granite, from the works of Messrs Macdonald and Leslie, has been exposed to all the vicissitudes of our changeable climate, for six or seven years, in the church-yard of Falkirk, and appears in the full lustre of its original polish, as if it were erected yesterday.

Fine specimens of granite monuments by the same artists may be seen in the noble new cemetery at Glasgow, which are chaste in design, beautiful in execution, and seem calculated to bid defiance to every destroying influence, except wilful injury.

On visiting the establishment of Messrs Macdonald and Leslie at Aberdeen, I saw several finished specimens, and many works of this material in progress, as I was conducted through the different departments, by the intelligent, and most respectable head of this interesting and new employment of national art and industry.

The grey granite is of a close grain, and contains more mica than the red. It is brought from quarries on the Dee, a short way above Aberdeen. The red granite is of a larger grain, abounding in felspar and in quartz, intermingled with small specks of mica, and bears a strong resemblance to the syenitic rock, of which the finest ancient Egyptian monuments are fabricated. This comes from the vicinity of Peterhead, and is brought by sea to the works. Both are susceptible of a fine polish, which they retain unimpaired by the weather. Blocks of almost any size may be obtained free of flaws or imperfections. In the sawing room, several blocks were then under the machines, which are moved by a 14-horse power steam-engine. I observed one block, 10 feet long, cutting into 6 or 8 slabs. The saws are, as usual in such works, of soft iron-plates, secured in a frame; and operate on the stone by means of quartz-sand and water, applied as in slicing marble. No emery is requisite in these operations, the particles of siliceous sand being sufficient to cut the quartz, the hardest material in the granite. Frequently 14 saws are used in a single frame; and occasionally they have had as many as 18 employed at once on a single block of stone. The progress of the work, of course, is slow; it requiring a whole day to cut a groove two-thirds of an inch in depth in the granite. The slabs, when cut, are polished by moving one over the other, by appropriate machinery; siliceous sand being first interposed, and then emery of various degrees of fineness, until the requisite degree of lustre is obtained.

The first dressing of the granite blocks into parallelopipeds, cylindrical masses or other curved forms, is performed by *hand-picks*, with short handles, and heads about 4 pounds in weight; which the workmen, from long habit, wield with surprising accuracy. The surfaces are then reduced to a regular form by means of well tempered chisels, urged by iron mallets; the chisels require a very particular temper, which must be neither very hard nor very soft, else they would either lose their edge by *chipping*, or fail to cut the stone. I observed that they frequently require sharpening in the more delicate kinds of work. The chisel is held by the workman very obliquely to the surface of the stone, and he separates very small particles at a time.

I have already described the polishing of plane surfaces. Circular forms, such as *stelæ*, frusta of columns, as pedestals for busts, vases, and the like, are fixed in well-contrived lathes, and are whirled round by machinery, while the sand and emery are applied to their surfaces by means of thick plates or bars of iron, previously forged to their various curvatures, when they are not cylindrical.

I saw a large vase, about 4 feet in diameter, prepared by the chisel for the process of polishing. Its graceful curves were beautifully and accurately cut by the chisel; the iron bars, 1 or 1¼ inch in thickness, neatly forged to its various curves, lay beside it ready to be applied, when it was fixed in the lathe.

In the warerooms were many finished articles of great beauty and elegance, such as well executed pedestals for busts or vases, of red and grey granite ; chimney pieces of the same material, numerous slabs, tables and seats for halls, and beautiful vases, in a considerable variety of forms, rivalling those of classic Italy in shape, mural tablets for monuments, and some altar-formed tombs of magnificent size. These last were made to order. Some of the chimney-pieces are intended for the Earl of Lauderdale's residence, Thirlstane Castle, and some of the slabs for Sir Robert Peel, &c. &c.

I was surprised at the neatness of the *lettering* on all the monuments ; and saw the men at work. The monument is first finished in other respects : the letters are carefully traced with a dark or light crayon, according to the colour of the stone, and the workman traces the outline of the letter on the stone by light strokes of a fine-edged chisel, held nearly vertically ; deepens the lines by a succession of similar blows, while the chisel is held very obliquely, removing the stone in the state of powder, so as to avoid chipping. Roman capitals are thus easily formed ; but I saw old English, or German letters, with a superfluity of curved lines, carved on the granite with equal precision.

But the most remarkable work which I saw in this establishment was, the neatly finished statue of the late Duke of Gordon, intended to be erected in one of the streets of Aberdeen. It is 11 feet high, of a single block of granite. This statue was modelled by Mr Thomas Campbell, the sculptor ; and has been transferred from the model to the granite by Messrs Macdonald and Leslie. Two men were at work on the drapery, at the period of my visit. They worked with fine chisels, held very obliquely, and urged on by iron mallets of two or three pounds in weight. The attitude of this statue is simple, and the features are said to be very like the original. This, which may be considered as the first specimen of a British statue of a single block of granite, in emulation of the durable monuments of ancient Egypt, is a memorial by the County to the late noble and gallant Officer ; and, when erected, will be a distinguished ornament to Aberdeen.

Another great public work, executed by the same artists, is already erected in that town. In 1842, the splendid public markets of Aberdeen, excelled by none in Europe in elegance, were first opened. The great saloon, containing the fruit and vegetable market, a magnificent hall 300 feet in length by 100 feet in breadth, has within it a noble fountain of highly polished Peterhead granite. An octagonal basin, constructed of polished blocks, stands about one-third the length of the hall from the southern extremity. From the centre of this basin, rises a shaft 10 feet high, supporting two circular cups or shallow vases, one placed over the other. The lowermost is formed out of a single block, 7 feet 3 inches in diameter; and the upper has about half that width. A constant jet of water rises from the centre of the upper cup, flows over its edges into the lower vase, which also overflows, in a thin sheet of limpid water, into the basin below; whence water is drawn for all the purposes of the market. I have seen no fountain in Britain so fine as this. It resembles in form, and surpasses in material, the finest fountains I saw in Spain: yet it was erected by Messrs Macdonald and Leslie for L.200.

The same artists are at this moment engaged in executing a similar fountain for Lord Prudhoe, which, I understand, will cost about L.200.

Indeed, considering the difficulty of working so hard a material, I was surprised at the moderation of their prices, for articles produced at their interesting establishment.

For instance :—

1. A hall-table slab of polished granite, measuring 4 feet long by 21½ inches wide, costs L.4, 15s.

It may be stated, that slabs may be furnished, of any required size, for from 12s. to 14s. for each square foot of surface.

2. Pedestals for busts, square or columnar, with plinth, and an ovolo when columnar, of the usual size, for L.10.

3. Mural monumental tablets, with vase, trusses, &c., from L.6 to L.9, according to the size.

4. Mural tablets, with base, cornice, and pedimented top, from L.10 to L.12.

Lettering, of the usual size, is charged 4s. 6d. per dozen of letters.

5. An elegant *Tazza-formed* vase, of classic shape, 4 feet

9 inches in diameter, and standing 2 feet 9 inches high on a beautiful pedestal, costs L.40.

6. They have also executed columns of granite for halls and vestibules, at prices equally reasonable, in proportion to the size and style of decoration. But of all the purposes to which they have hitherto applied the granite, it seems especially suitable for monuments of every kind, both from the beauty of the highly polished material, and its imperishable nature under all vicissitudes of the weather.

The extent and perfection to which these gentlemen have carried the working of this very refractory but beautiful stone, may be considered as forming an era in British art; and require only to be more generally known, to be appreciated and encouraged by public taste and munificence.

13 GLOUCESTER PLACE, EDINBURGH, *March* 18, 1843.

Researches on the Comparative Anatomy of the Chimpanzee.

By M. VROLIK.*

If, in the natural sciences, the study of facts ought to serve as the basis of general views and as the means of appreciating natural phenomena taken as a whole, the investigations undertaken with the view of throwing light upon some particular points of science, and supporting in some way or other a special object, deserve more than ordinary attention. The physical sciences geology and botany, present us with numerous examples of these *monographs*, many of which have been the means of acquiring the highest reputation to their authors. Zoology, and, in particular, Comparative Anatomy, are less rich in works of this description; it is therefore a duty to notice particularly works which, like M. Vrolik's *Researches on the Comparative Anatomy of the Chimpanzee*, combine a profound study of the subject with new views and ingenious speculations; the more especially when, in addition to these recommendations, the mode of execution, the form, and the plates, are such as to render it worthy of a place by the side of the most remarkable works of the class to which it belongs.

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* The valuable work noticed above is entitled, Recherches d'Anatomie Comparée sur le Chimpansé. Par W. VROLIK, Chevalier de l'Ordre Militaire de Guillaume, Membre de la Première Classe de l'Institut Royal des *Pays-Bas.* *1 vol. fol.* avec 7 Planches. Amsterdam, 1841.

M. Vrolik enters into no details either respecting the external characters or natural history of the Chimpanzee. Supposing these to be sufficiently known, he devotes himself to the anatomical examination of this animal, which is rendered singularly interesting by its great resemblance to man. Availing himself of the advantages supplied by the fine anatomical collections of Holland, both public and private, as well as those offered by the Zoological Garden of Amsterdam placed under his direction, he has added anatomical observations on many other species of monkeys, compared their organization with that of other quadrupeds, and contrasted it with that of man, in such a manner, that the work we now introduce to the notice of our readers almost amounts to a treatise on the comparative anatomy of the quadrumana, and a pretty complete essay on the comparative myology of the Mammifera.

A work of this nature, whose merits depend chiefly on the number and exactitude of its details, cannot easily be subjected to analysis. A few quotations of general interest will render it best known, and will, we doubt not, excite the desire of studying, in the work itself, the very peculiar organization of the large quadrumanous animal in question. The seven beautiful lithographic plates which accompany the descriptions, render them, besides, much more intelligible.

After long and interesting details respecting the osteology and myology of the Chimpanzee, as well as the comparison of the organs of motion among different species of monkeys and other mammifera—between these and the corresponding parts of man—the following are the general considerations arrived at by the Amsterdam Professor :—

" In short, it appears proved that the muscles of the anterior extremities become simplified in proportion as animals recede from the human form. Their number and disposition are modified according to the functions for which these anterior extremities are adapted. In man they are not intended to support the body. In him they are attached in such a manner, from the top of the head to the heel, that there is no part of the individual to which they cannot reach. By the nature of this attachment, and by all the peculiarities of their structure, we perceive that they are given to him as instruments adapted either for pushing away from him, seizing, or embracing objects, and, in particular, as organs of touch. It is to the hand, in particular, that the duty of fulfilling these offices is assigned. Every thing concurs, in man, to render it an organ of the greatest perfection, and in this respect no animal can rival him. Let us observe, accordingly, that it is for the purpose of executing these different functions that the palm is enlarged, radiating, and terminating in fingers, each phalange of which has its proper motor; that the thumb has a different direction from the other fingers, is not placed on the same line with them, but can be opposed to each of them; that the hand not only exercises a movement of extension and flexure, but can be turned forwards and backwards, by a mechanism peculiar to the wrist; that the articulation of the shoulder is formed in such a manner that the movements of the humerus, and consequently all the upper extremity, become as extensive as possible; that the muscular sides of the palm are so disposed, that the hand can form the palm into a hollow. All these arrangements are found in the greatest perfection in man, and the first result of them is, that he has the power of seizing an object with only one hand, while the other mammifera, whose fore-feet have some resemblance to the upper extremities in man, cannot hold objects but by using both hands. To this monkeys are the only exception. In them the fore-foot resembles the human hand, although it is very inferior to the latter. The palm is longer, and not so broad; the fingers are more elongated, and less insulated in their movements; the thumb is placed farther backwards, and, in its direction, less opposed to the fingers. Among them, consequently, the hand becomes less an organ of touch and prehension, than a means of aiding them in their movements while climbing trees. This imperfection is seen in its greatest degree among the *sapajous* and *sajous*. This is perhaps the reason why they are possessed of an accessory organ of motion, formed by the prehensile tail. In the ourang-outang, on the contrary, and still more in the Chimpanzee, the hand makes a much nearer approach to that of man. Although pretty perfect in the ourang-outang, it exhibits in that animal a disproportionate length; but in the Chimpanzee the fingers are shorter, the thumb better formed, and the palm of the hand broader. I cannot determine whether the palm of the Chimpanzee can form a hollow, like that of man, but I have often satisfied myself that that of the ourang-outang is incapable of doing so. When the ourang-outang of our Zoological Garden makes use of his hand, whether it be to seize on any object, or in any of the artificial movements he is caused to execute, he does it with a certain degree of awkwardness, which demonstrates his inferiority in this respect as compared with man. The last director of our menagerie amused himself by making it dine at his table; but although it had learned to imitate all the movements of a civilized man, to present its empty plate, hold out its glass, and eat with a spoon, it sufficiently shewed that its hand would not allow it to attain the dexterity of man. For example, in taking a plate or any other object, it never held its hand extended and open, as a man does, but closed the hand, bending the fingers very much. This mode of curving the fingers was extremely familiar to it. I never recollect of seeing its fingers completely extended. All this shews us that the hand of the ourang-outang is well adapted to grasp the branches of a tree; that in this respect it is an organ of motion of great perfection, and in every respect appropriate to the animal's mode of life, but that, in all other respects, it is inferior to that of man. I remarked the same thing in the ash-coloured gibbons of our menagerie. This inferior degree of aptitude in the hand of animals to serve all the purposes which it fulfils in man, is owing to the disproportionate length of the fingers, and, in particular, the inferior perfection and the situation of the thumb. By the disposition of its muscles the thumb of monkeys is not made for that variety and great freedom of motion peculiar to man. Certainly that of the Chimpanzee approaches nearest the human thumb, and yet the great flexor muscle is sometimes wanting, and the smaller abductor and antagonist of the thumb are much less developed than in man. In the other monkeys, the great abductor and small extensor of the thumb are confounded, in so much that there appears there, as in all the other muscles of the anterior extremities, a great tendency to become simplified. In man they are undoubtedly most complicated ; in him also the movements they perform are most varied."

After the description and detailed comparison of the posterior extremities of the Chimpanzee and other Mammifera, we find the following considerations respecting these organs.

"By this comparative description of the myology of the posterior extremities, I think I have demonstrated that their muscles become simplified in animals in proportion as we recede from their perfection in man. And if we consider attentively what is peculiar and distinctive in the organization of these posterior extremities, we cannot doubt for a moment that they are destined to support and move the body. It is for this reason that the arrangement of their muscles is entirely different from that we have observed in the anterior extremities. For while we see the force of flexion prevail over that of extension in the anterior extremities, we witness, on the contrary, that of extension prevail over flexion in the posterior extremities. It is particularly in man that this fact is shewn in the most conspicuous manner. We have only to compare the development of the extensor muscles of the leg with that of the flexor muscles, to be convinced of this, or, if we wish a proof more conclusive still, we have but to examine the muscles of the leg. It is principally to the great strength of all these extensor muscles that man owes the power of holding himself erect and walking on two feet. We again find it, for that same reason, in animals whose trunk is straight, and whose movements are principally made with the hinder feet ; the examples of the kangaroo and sloth prove this. I do not add the example of the monkeys, because there is none of them that can hold itself upright and walk without any other support than the hinder feet. They are all quadrupeds, with this modification, that the four feet are but ill fitted to support and move the body on a horizontal plane, but rather for making it ascend a vertical plane. The movement they perform in the act of grasping is their true attribute. We have only to notice the manner in which they grasp the bars of their cage to be assured of this. Their feet are modified for the purpose quite in a peculiar manner, as I have fully stated in the osteological part of this work. And it is for the same reason that their muscles have the special character which I have assigned them in this chapter."

On the subject of the laryngeal pouches, the existence of which M. Vrolik has shewn in many species of monkey, he brings forward a new opinion as to their use. He supposes that these pouches " are organs fitted for facilitating motion. Their situation among the muscles of the neck, the prolongations which they often form in the arm-pits, their increase in size with age, appear to me so many proofs," he says, "that they are reservoirs of air, made for the purpose of diminishing the specific gravity of the upper part of the body, and consequently to facilitate the act of grasping, in the same manner as reservoirs of air in birds favour flight." *

On the Rein-Deer of the Laplanders. By Gustav Peter Blom, Member of the Royal Academy of Sciences of Drontheim, &c.

The Laplanders are originally a Nomadic race, supported by rein-deer, and their principal branch still follows the same mode of life. Poverty, however, has forced many Laplanders to quit their native haunts in the mountains, and to descend to the Norwegian coasts, or to the plains of Lapland, to seek for the means of living. Thus two kinds have sprung up in Norway: the *Sea-Laps,* who live on the coasts, and are occupied with fishing, and the *Boe-Laps,* who have settled in the valleys, have brought small tracts of land into cultivation, and support themselves by agriculture and the rearing of cattle, combined partly with the rearing of rein-deer. The Laplanders who have withdrawn to Lapland may again be divided into two kinds: the *Forest-Laps,* who keep reindeer, but take them along with themselves only within a certain region, and who at the same time are hunters; and the *Fisher-Laps,* who have established themselves on the shores of the great rivers and lakes of Lapland, and are engaged in the taking of fish. The best shots are among the Forest-Laplanders, who furnish the yearly markets of Vitangi and Kengis with a large quantity of game, which is carried to Stockholm by way of Torneo.

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* From Bibliothèque Universelle de Genève, No. 83, p. 170.

The rein-deer is the support of the Laplanders, and the object of their pride; in it consist their wealth and their happiness. Whoever is the possessor of many hundred rein-deer, has attained the highest pinnacle of good fortune; but he never on this account alters his mode of living in the slightest degree, or increases his enjoyments, except, perhaps, as regards the quantity of brandy he consumes. Besides the rein-deer, the whole wealth of the Laplander consists of few articles of clothing, his tents for living in and for keeping his stores, a few wooden stakes with which he forms a kind of fold, into which the rein-deer are driven when they are to be milked, a few bed-covers made of rein-deer skins, a copper vessel in which his food is cooked, a few wooden dishes, and his provisions, consisting of rein-deer-cheese and milk, which latter he preserves for the winter in rein-deer stomachs. When he alters his abode, the whole of this splendour is placed on the pack-rein-deer, and conveyed to the new place of residence.

The rein-deer is the most important possession of the Laplanders, for it supplies them both with nourishment and clothing. The Laplander spends his superfluous money chiefly on the increase of his herd; and it is only when that is sufficiently large, that he begins to think of collecting silver and burying it; but he never dreams of procuring greater personal comforts, for their value is unknown to him.

The Laplander lives in a tent of a circular conical shape, provided with an opening above for the escape of the smoke. The tent is made of coarse woollen cloth, sometimes also of rein-deer skins, and the richer individuals construct their habitations with a double covering. The door consists of a curtain of the same material. The internal arrangement of the tent is just as simple; in the middle there are a few stones which form a sort of fire-place, and at the sides round about, twigs of birch are strewed, and rein-deer skins spread over them, so as to form a sofa during the day, and a bed at night. The dogs also partake of this place of repose. The dishes and kettles lie scattered about in the tent, and above are suspended the rein-deer stomachs filled with milk, which are completely blackened by the smoke. It is to be expected that cleanliness should not exist in such miserable dwellings, but the Laplanders have in fact no idea of it. A few of the race, who pasture their rein-deer on the coasts every summer, have built earthen huts in the form of tents; but these have no advantage over their usual abodes.

It is only in autumn that the Laplander kills his rein-deer, for it is only at that season that they are fat, and their flesh palatable. In spring the rein-deer has much to endure from the so-called rein-deer fly,—an insect which penetrates into the skin of the animal, and deposits its eggs, from which larvæ are produced. The animal is thus so much tormented, that it becomes lean in summer, and the skin is of no value so long as the larvæ exist in it. The insects produce larger or smaller tumours on the backs and sides of the rein-deer, and the poor animals fall on their knees, on occasion of the slightest touch, in order to escape the pain. The female produces its young in the month of March, and from that time it is milked, by some of the Laplanders once, and by others twice a-day. The milking of the rein-deer is one of the most interesting scenes in the whole economy of the Laplanders.

Towards evening the rein-deer are driven from the mountains to the tents. Their arrival is first announced by the barking of the dogs, who run round the herd, to keep the animals together. Soon the whole herd is descried, forming a closely packed mass, which moves along like a grey cloud. As the animals approach nearer, the horns become a prominent object, resembling a moving leafless forest, and very various in their form and size. The fawns push through among the full-grown animals, and we at last hear a crackling noise, produced by the movement of their legs, and resembling the sound of burning fir-trees, or rather that of electric sparks. Here and there is heard a sound somewhat like the grunting of swine. Near the tents there is a circular enclosure, provided with two openings or doors. When the rein-deer approach it, they press closely together in order to enter, and one sees only the moving mass and the projecting horns. Should a deer or a fawn remain behind, or take a wrong path, a dog immediately pursues it, and the deserter is soon seen running back to the herd at full pace, followed by the dog. The animals now stand closely packed together within the fence, and are so tame that a stranger even can touch them without trouble or danger. In the centre of the enclosure there is a small erection to which the animal is strongly bound during the milking, in order that it may not become unruly, and upset both the milk and the milker. The milking is performed by men, women, and children; but the task of bringing the animals to the milking place belongs exclusively to a particular man, and is accomplished in the following manner:—

This individual is accurately acquainted with every animal, even in a herd of several hundred, and knows if it is a male or female, and if it is milked or not. He goes with a noose in his hand, and throws it so dexterously over the horns of the animal he wishes to secure, that he never fails in his aim, even at a distance of fifteen or twenty yards, and when many other individuals are standing between him and his object. So soon as the noose is fastened round the horns, the animal is dragged to the milking-place, and there securely tied; another animal is afterwards taken in the same way, and so till all have been milked. The skill of the Laplanders in the use of this noose can only be compared to that of the savages of Africa, or the bull-takers in Brazil.

But little attention is paid to cleanliness in the milking, and indeed generally in the economy of the Laplanders. During the summer, loose hairs fall abundantly into the milk, and these are but partially removed by sieves. The milk not used is poured into rein-deer stomachs and suspended in the tent. The rein-deer understands how to keep back the milk; and, in order to prevent her doing so, the Laplander often strikes her repeatedly with his fist, and thus much additional hair drops into the milk. But little milk is obtained; it is, however, as rich as cream, and the taste is by no means disagreeable, resembling that of the ewe. An exceedingly palatable cheese is prepared from it, which is used medicinally as a certain cure of boils produced by frost.

An important animal in the economy of the Laplanders is the dog, and every Laplander has a number proportionate to that of his rein-deer, amounting to twelve or more. These dogs protect the rein-deer from wild animals, gives a signal when these approach, keep the herd together, so that they may not become scattered, and thus lose themselves in the mountains, and go in search of them when the latter occurs. They drive the deer by their barking, but when that is not sufficient, they bite their legs. In order to prevent injury being thus inflicted, the canine teeth are extracted when the dogs are young. It is rather a natural instinct than a regular training which teaches the dogs their duty. They have a natural inclination to the rein-deer, and so soon as the latter are in motion, are ready to follow. The dogs are divided into two sections, of which the one accompanies the herd, and the other remains in the tents. As soon as the rein-deer return from their pasture to the tents, the dogs which have been reposing start up and enter upon their duties, and those which are thus relieved lie down quietly in the tents.

The Lapland dog is not large, has long hair, a sharp snout, a long-haired tail, and erect ears; it has no claims to beauty.

The domestic rein-deer are not always of a grey colour, like the wild, but vary in this respect like all domesticated animals. Although the prevailing colour is grey, there are rein-deer of a white colour with blue spots. For the most part they have white markings on the head and feet, by means of which they are recognized by the Laplanders, and by which the possessor can not only distinguish his own from strangers, but even every single animal in his herd.

Males only are used as beasts of burden, and chiefly those which are castrated, as they are the strongest. The female is too tender for such work. The rein-deer is most valuable for dragging, for its power of carrying is not great, and while its progress when loaded is slow, the burden must also be small. On the other hand, when the snow is in a good state, it drags large loads with great rapidity. As is well known, travelling in Lapland in winter is only performed by means of rein-deer, and is accomplished at a very quick pace. The horse is useless at this season, because there are no made roads, and no places for repose or feeding. Such accommodations are not required for the rein-deer; for it runs on the untrodden snow, and when unyoked from the sledge, it scratches the snow with its feet and refreshes itself with the moss, which it is always able to discover on the mountains.

The knowledge of locality is just as remarkable among the Laplanders, as their power of recognising their rein-deer, and arises from the same cause, viz., from the development of their senses and perception, which is promoted by the necessity that exists among them, as among all people in their natural state, for relying on themselves for extrication from difficulties. Although the Alps of Lapland, and more especially the plains, offer but few objects which can fix attention, there is no example of a Laplander losing himself on a journey; if he has once travelled over a tract, it becomes known to him for his whole life. Fog alone, or drifting snow, can lead him into error; but he takes good care not to travel in such weather, and his meteorological knowledge enables him to foresee when anything of the kind is to be dreaded. His acuteness of vision allows him to descry objects at very great distances, and thus to pilot himself. His eyes, however, become weakened at an early period, owing to the smoke in his tent, and partly to the dazzling whiteness of the snow. When a Laplander is caught, during a journey by night or a storm, he throws his *kaftan* over his head, lies down on the snow, and covers himself with it, waiting patiently for a more favourable opportunity of prosecuting his journey.

The mode of living of the Laplanders is simple in the highest degree, especially in summer; for at that season they are supported almost exclusively on rein-deer milk, and a kind of sorrel, which they find in abundance in the mountain valleys, and cook along with milk in an uncoated copper vessel, without, on that account, suffering bad effects in the stomach. Fish are very welcome to the Laplanders, but are a dainty which they do not often enjoy, as the Alpine Laplander occupies himself but little with fishing. A favourite kind of food is the stalk of the *Angelica archangelica*, here named *slöcke*, which the Laplander eats raw, after removing the outer fibres. This plant is also much eaten by the Northmen, and is considered as a good preservative against scurvy.

Meal is not used in summer; but in winter, the Laplander exchanges his rein-deer flesh for meal in the markets and coast districts; and he then eats the flesh, or the preserved milk, cooked with meal, or a kind of soup made of rein-deer blood and meal. His food in winter is very nourishing, and it is thus that he is able to endure the hardships and severe weather with which he has to contend.

Many travellers, and among them Brooke,* have asserted, that the Laplanders proceed yearly with their rein-deer to the coasts of Norway, and that it is a matter of necessity that the animals should drink sea-water every year; but this is not the case. The wandering of the Laplanders is by no means regular, and many rein-deer—nay, the greater number—have never tasted sea-water. It entirely depends on the locality, whether the Laplander goes to the sea-coast or not, and whether this takes place in summer or winter. In the districts Namdalen and Senjen, whose coasts are surrounded by islands having high cliffs, the Laplander drives his rein-deer to the coasts, and thence takes them to the islands in order to procure food for them. This transport presents an interesting spectacle. The Laplander attaches one or several rein-deer to his little boat by means of a rope, which is secured round the horns. He then rows across the sound, which is often more than an English mile broad; and the rest of the animals having been driven into the sea, swim after their leaders to the opposite coast. In other localities, the Laplander goes to the coast in the winter season, when the snow is too deep on the mountains, and he again quits it in April or May. In a valley, an English mile or two from the town of Tromsöe, a Laplander remains till the beginning of August, with 700 rein-deer. It is evident, from what has now been said, that no particular natural impulse takes the rein-deer at fixed seasons to the sea; on the other hand, it is an undoubted fact, that the rein-deer will not remain longer than about the end of August in the coast regions and in the Norwegian pastures—nay, that if the Laplander does not hasten, before the 20th August, towards the mountains, his herd will desert him, and proceed on their journey to the plains of Lapland.

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* For a portion of Brooke's Account of the Rein-Deer, see Jameson's *Journal*, vol. iii., p. 30.

The wanderings of the Laplanders generally take place in the following order: In winter, they remain partly in the vast moorish tracts, partly in the forests of Lapland; and in spring, the torment caused to the rein-deer by gnats and rein-deer flies, forces them to remove to the Norwegian confines, where these insect-enemies are less troublesome, and where the animals may enjoy the snow. Some Laplanders proceed to the valleys, and to the islands near the coast. In autumn, they return to the Lapland plains. In some districts, they spend the winter in the Norwegian Alpine valleys; but so soon as the snow drives them away, they seek the coasts, until the spring again renders the Alps passable. The Laplander always pitches his tent in the neighbourhood of a forest, in order to obtain fuel; while in summer, the presence of a river or a spring, is a necessary condition in the choice of a residence—melted snow supplying the necessary water in winter.

The fondness of the Laplanders for silver money is well known, and it is only those who have intercourse with the inhabitants of the coasts who take paper money. It is asserted, that they are still in the habit of burying their money in the mountains, which is easily understood, when we consider, on the one hand, their timidity and mistrust; and on the other, that it must be extremely difficult for them to carry articles of value about with them during their constant wanderings. The natural consequence is, that considerable sums are lost among the mountains, as death frequently surprises the Laplander before it is possible for him to reveal to his relations the spot where the treasure is buried; and as it is not possible to indicate it without being actually at the locality—a circumstance which does not often occur.*

I. *Connection of the Physiognomy of a Country, with the Character of its Inhabitants.—I. Belgium.—II. Holland.—III. Midnight Scene on the Ocean.—IV. A Scene in Norway.*

I. Belgium.

Mr Trollope indulges in much censure of the manners and morals of the Belgians, and commits the customary blunder of English travellers, in imputing the extortions of tradesmen to the character of the people. The Belgians have always appeared to us remarkable for stolidity and plodding industry, without much refinement of mind or feeling, or, on the other hand, any extreme stupidity or coarseness. They are, in our judgment, a race deficient in marked features of character, rather than obnoxious to the imputation of any prominent vice. Without pretensions to high virtues, they are generally exempt from characteristic crimes. Whether there is any natural connection between scenery and character, we will not undertake to pronounce; *but a striking analogy prevails between the productive flatness of the land and the utilitarian mind and capacity of the inhabitants.* It is no uncommon thing, especially in Flanders, to see four miles of road with a strip of pavement in the middle, and a ditch on each side straight before you, and a dead level right and left as far as the eye can reach. The land, if it be in summer, is blooming with bean blossoms, or gilded with the rich and ripening corn; and very agriculturally interesting it doubtlessly is, to see so much goodly produce and evidence of fertility; but where the land is a dead flat, and roads and trees run in perfectly straight lines, it is tiresome work to travel there, and very soporific. To be sure, one does occasionally see a church at the end of an interminable looking road. You watch it (for it forms a pleasing variety in the landscape), gradually developing itself, as you jog nearer and nearer to it, till at length its form, then its shape, its colour, its weathercock, and its cherubed waterspouts, one by one appear; and at last the grim countenances of the weather-beaten saints scowl out of their niches at you as you pass; you then make a slight turn, and another long flat line opens upon you. The lives of the Flemish women are, at any rate, akin to the intense sameness and monotony of scenery; and Mr Trollope's description is not very wide of the truth. A Flemish wife rises in the morning and drinks her coffee, dresses the children and herself, sends the former to school, and goes to market, where the entire mental exertion of her life centres; and something faintly approaching energy and animation is observable as she higgles in succession with the poultry woman, the fruit and vegetable women, the butcher, and the egg merchant. If she be of the easy class, her servant follows and baskets the purchases as the mistress makes them. When completed, she repairs forthwith home; or if she has no servant, with basket on her arm, goes to church and says her prayers. The personal superintendence of the preparations for dinner occupy her till noon, when the husband returns; and that great event of the day having been achieved, and the children, if any, been again dispatched to school, the knitting-needles are plied incessantly till evening, enlivened by a cup of coffee at about four o'clock. When the husband returns, occasionally in summer half an hour's walk is indulged in, or they visit a garden, where the husband smokes and the wife not unfrequently knits. Supper is served at seven, the children are sent to bed, and the wife, after another batch of knitting, follows at nine or ten o'clock, having performed her functions much after the fashion of *the clock*, by whose mechanism her own movements are regulated. A more mindless set of women it is difficult to find. Their virtues consist in docility, evenness of temper, and domesticity.—*Athenæum*, No. 779, p. 848.

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* From Blom's *Königreich Norwegen statistisch beschrieben.* 1843.

II. *Holland.*

Holland, the land of cheese and butter, is, to my eye, no unpicturesque, uninteresting country. Flat it is; but it is so geometrically only, and in no other sense. Spires, church-towers; bright farm-houses, their windows glancing in the sun; long rows of willow-trees, their bluish foliage ruffling up white in the breeze; grassy embankments of a tender vivid green, partly hiding the meadows behind, and crowded with glittering gaudily-painted gigs and stool waggons, loaded with rosy-cheeked laughing country girls, decked out in ribbons of many more colours than the rainbow, all a-streaming in the wind; these are objects which strike the eye of the traveller from seaward, and form a gay front view of Holland, as he sails, or steams along its coasts, and up its rivers. On the shore, the long continuity of horizontal lines of country in the background, each line rising behind the other to a distant, level, unbroken horizon, gives the impression of vastness and of novelty. Holland can boast of nothing sublime; but for picturesque grounds, for close, compact, snug home scenery, with every thing in harmony, and stamped with one peculiar character, Holland is a cabinet picture, in which nature and art join to produce one impression, one homogeneous effect. The Dutch cottage, with its glistening brick-walls, white painted wood-work and rails, and its massive roof of thatch, with the stork clappering to her young on her old-established nest on the top of the gable, is admirably in place and keeping, just where it is, at the turn of the canal, shut in by a screen of willow-trees, or tall reeds, from seeing or being seen, beyond the sunny height of the still calm water, in which its every tint and part is brightly repeated. Then the peculiar character of every article of the household furniture, which the Dutch-built house-mother is scouring on the green before the door so industriously; the Dutch character is impressed on every thing Dutch, and intuitively recognised, like the Jewish or Gipsey countenance, wherever it is met with; the people, their dwellings, and all in or about them, their very movements in accordance with this style or character, and all bearing its impress strongly—make this Holland, to my eye, no dull unimpressive land. There is a soul in all you see; the strongly marked character about every thing Dutch pleases intellectually, as much as beauty of form itself,—what else is the charm so universally felt, requiring so little to be acquired, of the paintings of the Dutch school? The objects or scenes painted are neither graceful, nor beautiful, nor sublime; but they are Dutch. They have a strongly marked mind and character impressed on them, and expressed by them, and every accompaniment in the picture has the same, and harmonizes with all around it. The Hollander has a decided taste for the romantic; great amateurs are the Mynheers of the rural. Every Dutchman above the necessity of working to-day for the bread of to-morrow, has his garden-house (Buyteplaats) in the suburbs of his town (for the Dutch population live very much in town surrounded by wet ditches), and repairs to it on Saturday evening with his family, to ruralize until Monday over his pipe of tobacco. Dirk Hatterick, we are told, did so—it is the main extravagance of the Dutch middle-class man, and it is often an expensive one. This garden-house is a wooden box gaily painted, of eight or ten feet square; its name, "My Delight," or "Rural Felicity," or "Sweet Solitude," stuck up in gilt tin letters on the front; and situated usually at the end of a narrow slip of ground, enclosed on three sides with well-trimmed hedges and slimy ditches, and overhanging the canal, which forms the boundary of the garden-plot on its fourth side. The slip of land is laid out in flower-beds, all the flowers in one bed being generally of one kind and colour; and the brilliancy of these large masses of flowers, the white and green paint work, and the gilding about the garden-houses, and a row of those glittering fairy summer lodges, shining in the sun, upon the side of the wide canal, and swimming in human brilliancy in the midst of plots and parterres of splendid flowers, and with the accompaniments of gaily dressed ladies at the windows, swiftly passing pleasure-boats with bright burnished sides below, and a whole city population, afloat or on foot, enjoying themselves in their holiday clothes, form, in truth, a summer evening scene which one dwells upon with much delight. I pity the taste which can stop to enquire if all this human enjoyment be in good taste or bad taste, vulgar or refined. I stuff my pipe, hire a boatman to row me in his schuytje up the canal to a tea-garden, and pass the evening as Dutchly and happily as my fellow-man.—*Laing's Notes of a Traveller.*

III. *A Midnight Scene on the Ocean.*

One more of the beautiful and poetical pictures which Professor Steffens paints with so vivid yet so soft a touch—once more let us rock our imaginations on the bosom of the deep, before we go back to the world of men and things. We know of few attempts in prose or verse to describe the undescribable, the awful majesty, and the profound, mysterious attraction of the ocean, equal to the following. Our author was good-naturedly invited by a party of six fishermen to accompany them on an expedition to a sand-bank, at a distance of six or seven Norwegian miles from shore, where they were to pass the night. They sailed in a serene and beautiful morning: the wind afterwards rose, and the sea was agitated.*

"The night I passed there I shall never forget. As twilight closed around us on the tossing waves, we became more and more silent; the masts were lowered; the fishermen were contented with their day's work, and I now threw out my net once more; the kind-hearted fellows pressed round me with friendly curiosity as I emptied my rich booty into the tub, and began to examine it. I had to give a popular lecture on the new and rare productions I had caught. Meanwhile, though the sun had sunk below the horizon, the bright evening red remained visible the whole night in the far west, and played on the waves around us — now gleaming, and then vanishing like a soft lightning. The oars lay still; the boat, left to itself, rocked on the waves; the conversation fell into monosyllables; my companions sung a hymn; I heard the murmur of their prayers, and then each, folding himself in his cloak, lay down to sleep: they slept the deep sleep of tired men. The billows dashed against the boat, and the night-air closed over our heads; the consciousness that a fathomless abyss might at any moment swallow up our small bark kept me awake, and the power of the wondrous ocean—Solitude took possession of me. It was as if I belonged to the deep whose inhabitants I had disturbed with my daring curiosity. The dim horizon of my precarious future—a thousand pictures of the past, appeared and vanished again. Neither sorrow nor joy could assume a distinct form; all feelings blunted each other—all images rocked like the boat, and melted into each other like the waves: it was a feeling such as I never experienced before or since. In the twilight, I could not discern the distant shore; and here I learned the deep, unfathomable might with which Nature rules the soul—here, as in no other situation. By degrees all images became dimmer and more shadowy—the rocking motion of my thoughts more tranquil, gentle, and calm; the plashing of the waves sounded like a lullaby, and I sank, like my comrades, into a deep sleep."—***Steffens**, in his " Was ich erlebte."*

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* British and Foreign Review.

IV. *A Scene in Norway.*

"In one of these wanderings, I remember," says Steffens, "to have spent the night in a valley so entirely shut in on all sides by naked, abrupt, precipitous rocks, that the sun was only visible a few hours in the middle of the day. A hut of unusual neatness stood in this valley; the grass was fresh, green, and luxuriant, from constant moisture; oats and barley were growing in sheltered spots; a few cows were feeding in the little meadow: everything breathed repose and comfort. The inhabitants of this peaceful nook—a hale, active old man, with a white beard, a good-natured old woman, a married son, with his wife and children—were so cordial, so delighted at the rare event of a visit from a young traveller, that I determined, after seeing the early setting of the sun, to stay for its late rising.

"The old people had not left their valley for years; the young woman had seldom been as far as the shore of the island. The son alone sometimes made journeys of business as far as Bergen; but these were by no means frequent, and their peaceful lives flowed on in the most complete seclusion. The incident made an indelible impression on my memory; because I never had so near a view of the riches of an apparent uniformity of life—of the completely enclosed tranquil fountain of a simple existence, cut off from all turbid and stormy waters, as here. Both father and son had been seamen in their youth. They had seen the world; knew France, Spain, and the ports of the Mediterranean, as sailors know them; they had carried back into their lonely valley a general picture of the relations of the external world; but the old man had lived here very long, and even the son for more than ten years. The events that then convulsed the world lay at an immense distance. Intervals, whether of time or space, appeared to have lost their significancy; and even the events of their own country and neighbourhood were as strange to them, and seemed as entirely severed from their own existence, as the events of the most distant lands. And yet these remote things were as vividly present to their simple minds, and affected their transparent souls as deeply, as if they belonged to their own most intimate being. As the infant stretches out its hands to grasp the most distant object as if it were to close it, so did their warm guiltless hearts embrace the remotest events as if they regarded themselves. They asked me a thousand questions. The whole existence and mind of these people was of such a limpid clearness, that I knew in a moment what incidents to relate, and how to describe them. Never had I a more attentive audience—never did I hear sounder judgments. The time passed with extreme rapidity in this soft physical and mental twilight, and yet, when I left the hut, I felt as if I quitted a long-accustomed home."—*Steffens, in his " Was ich erlebte."*

Meteorological Tables for 1842.

EAST SIDE OF SCOTLAND.

MR R. D. PAUL's Table.

METEOROLOGICAL TABLE, kept at Edinburgh, in North Latitude 55° 57′ 20″.

1842.	½ 8 A.M.		8 P.M.		Mean Temperature by Six's Thermometer.			Rain.	Snow.	Hail.	No. of Days Fair.
	Barom.	Ther.	Barom.	Ther.	Max.	Min.	Mean.				
Jan.	..	...	...	...	45°	20°	33.40°	4	12	4	17
Feb.	...	...	29.621	...	52	22	38.89	10	3	5	17
March	...	...	29.506	...	59	20	43.23	22	6	4	8
April	...	...	30.026	...	68	33	47.84	5	1	1	24
May	29.691	52.89°	29.705	51.00°	67	40	53.52	19	...	1	12
June	29.855	56.20	29.817	51.30	76	45	58.04	16	...	...	14
July	29.748	56.93	29.753	58.96	77	46	57.88	14	...	1	17
August	29.802	59.25	29.833	58.16	73	43	59.88	14	...	...	17
Sept.	29.729	54.33	29.744	53.60	69	41	54.81	18	...	...	12
October	29.780	45.38	29.770	44.41	60	28	45.41	8	2	2	22
Nov.	29.543	39.83	29.574	39.20	53	27	39.63	13	3	2	16
Dec.	29.683	44.00	29.700	45.03	55	28	44.30	22	1	2	11
Average	*29.728*	51.10°	29.731	50.20°	63.00°	33.58°	48.06°	165	28	22	18

ANNUAL RESULTS.

MORNING.

BAROMETER. Observations.	Wind.	THERMOMETER. Observations.	Wind.
Highest, 7th January, 30.38,	NW.	Highest, 10th August, 66°,	NW.
Lowest, 25th November, 28.65,	E.	Lowest, 6th January, 20°,	NE.

EVENING.

Highest, 8th October, 30.37,	NW.	Highest, 13th June, 70°,	N.
Lowest, 22d October, 28.62,	W.	Lowest, 23d November, 29°,	W.

EXTREME COLD AND HEAT BY SIX'S THERMOMETER.

Coldest, 6th January,	Wind NE.	20°
Hottest, 23d July,	Do. NW.	77°
Mean temperature of the year 1842,		48.06°

WEATHER.	DAYS.	WIND.	TIMES.
Fair,	187	N. and NE.	47
Rain, &c.	178	E. and SE.	72
		S. and SW.	96
	365	W. and NW.	140

Barometer during the first four months of the year taken at noon, and the mean height for that time is 29.850 inch.

The aurora borealis was observed but six times during the year, viz., on the 1st and 15th February, 3d and 12th April, 18th July, and 26th October.

METEOROLOGICAL TABLE.

Extracted from the Register kept at Kinfauns Castle, 56° 23′ 30″ N. L.

1842.	¾ PAST 8 A.M.		8 P.M.		Mean Temperature by Six's Thermometer.			Depth of Rain in Garden.	No. of Days	
	Barom.	Ther.	Barom.	Ther.	Max.	Min.	Mean.		Rain or Snow.	Fair.
Jan.	29.823	32.41	29.821	32.48	44	12	33.35	2.41	13	18
Feb.	29.632	37.71	29.616	37.25	52	27	39.28	1.91	11	17
March	29.525	40.00	29.499	39.32	55	29	41.48	2.67	14	17
April	30.013	43.63	30.023	42.26	64	28	44.70	...	...	30
May	29.685	50.22	29.735	48.19	65	36	51.38	2.10	14	17
June	29.804	57.40	29.738	55.46	76	41	55.86	1.33	11	19
July	29.739	57.22	29.730	56.22	77	39	56.58	2.89	11	20
Aug.	29.842	58.15	29.829	58.03	80	39	60.19	1.80	6	25
Sept.	29.741	54.56	29.762	50.40	68	38	54.16	1.42	10	20
Oct.	29.766	43.00	29.749	39.38	64	23	43.70	0.85	4	27
Nov.	29.582	38.96	29.580	37.53	58	25	39.96	2.78	11	19
Dec.	29.635	42.12	29.707	39.87	57	29	42.74	2.94	17	14
Average	29.732	46.29°	29.732	44.69°	64.16°	30.50°	46.94°	23.10	122	243

ANNUAL RESULTS.

MORNING.

BAROMETER.		THERMOMETER.	
OBSERVATIONS.	WIND.	OBSERVATIONS.	WIND.
Highest, 7th January, 30.41,	N.	Highest, 13th August, 64°,	W.
Lowest, 23d October, 28.56,	N.	Lowest, 15th January, 23°,	E.

EVENING.

BAROMETER.	WIND.	THERMOMETER.	WIND.
Highest, 7th January, 30.40,	N.	Highest, 31st July, 65°,	N.
Lowest, 22d October, 28.56,	W.	Lowest, 15th January, 24°,	E.

EXTREME COLD AND HEAT BY SIX'S THERMOMETER.

Coldest, 16th January,	Wind, E.	12°
Hottest, 18th August,	Do. S.	80°

Mean Temperature of the year 1842, ... 46.94°

WEATHER.	DAYS.
Fair,	243
Rain, &c.	122
	365

WIND.	TIMES.
N. and NE.	45
E. and SE.	125
S. and SW.	99
W. and NW.	96
	365

The amount of rain last year at Kinfauns Castle was 31.10 inches; whereas during 1842, but 23.10 inches fell: the difference being 8.00 inches. During 1839, a great quantity of rain fell, but was succeeded by a very dry year; but the quantity of rain, even then, was greater than during 1842. The mean temperature of 1841, by the Kinfauns Register, was 45.88; 1842 warmer by 1.06°.

RESULTS OF A METEOROLOGICAL JOURNAL, KEPT AT HARRABY, NEAR CARLISLE, BY JOSEPH ATKINSON, IN 1842.

BAROMETER.

Mean height at 9 A.M.	29.840
Mean height at 9 P.M.	29.821
Mean height of both	29.826
Highest A.M. on the 8th Oct.	30.509
Lowest A.M. on the 25th Nov.	28.659
Highest P.M. on the 9th April	30.563
Lowest P.M. on the 23d Oct.	28.465

THERMOMETER.

Mean of Maximum	53.5
Mean of Minimum	43.5
Mean of both	48.5
Highest, on the 18th August	81.3
Lowest, on the 21st October	14.8

RAIN.

Total quantity	21.825
Average quantity for the same month for seven years	33.056
Number of days on which rain fell	186
Average number of days for seven years	224

WIND.

NUMBER OF DAYS.

N. ... 14¼	E. ... 22¼	S. ... 32½	W. ... 36
NNE. 4¼	ESE. 3½	SSW. 24¼	WNW. 12¼
NE. 36¼	SE. 40¼	SW. 70¾	NW. ... 16¾
ENE. 6¾	SSE. 23	WSW. 20½	NNW. 1¾

	DAYS.
Total Easterly	150½
Total Westerly	214½
Calm	81
Moderate	209
Breeze	38
Strong Breeze	26
Stormy	11

WEATHER.

Clear	47
Sun shone out	319
Cloudy	132
Rain, 171; Snow, 15 =	186
Frost	58
Thunder	8
Hail	16

Alford Meteorological Table for 1842. By Dr Farquharson, LL.D., F.R.S.E.

Mean Results of the Thermometer, and the quantity of Rain, for 1842, at Alford—about Lat. 57°13 N.; 420 feet above the level of the sea, and 26 miles inland from the Sea at Aberdeen. Also, the number of fair days, and of days on which Rain or Snow fell, more or less.

The Thermometer was registered at 8¾ h., A.M., and 8 h., P.M.; and the daily extremes, at the latter hour.

1842.	Mean of Morn.	Mean of Even.	Mean of Morn. and Even.	Mean of daily highest and lowest.	Highest in the Month.	Lowest in the Month.	Rain in Inches.	Number of Fair Days.	Number of Rainy Days.
Jan. ...	32°.48	33.71	33°.09	33°.4	43°	17°	1.8	17	14
Feb. ...	35.39	37.14	36.26	37.88	49	18	2.55	19	9
March	39.74	40.35	40.045	40.53	58	31	2.25	17	14
April...	43.53	45.06	44.28	45.8	66	29	.7	27	3
May ...	51.06	51.25	51.155	51.19	68	36	1.25	15	16
June ...	56.8	56.7	56.75	55.49	74	38	3.2	15	15
July ...	56.35	56.25	56.3	55.77	72	40	2.875	17	14
Aug. ...	59.64	60.	59.87	59.59	73	41	1.45	19	12
Sept. ...	53.13	53.13	53.13	53.58	70	34	3.7	14	16
Oct. ...	43.12	43.8	43.46	43.93	60	18	5.2	19	12
Nov. ...	38.	38.1	38.05	38.46	55	26	7.1	12	18
Dec. ...	42.64	43.87	43.25	43.91	56	27	1.15	23	8
	45.99	46.61	46.3 Mean of Year.	46.62			33.225	214	151

Mean Temperature of ten years, ... 45°172
Mean Rain in eight years,........................ 36.793 Inches.

The year, as a whole, has been unusually delightful, without any of the extremes of heat or cold. The crop has been about an average one in grain, and safely got in; but the fodder somewhat deficient, owing chiefly to the drought of April, May, and the first part of June. The hay, owing to the same cause, was very light, and in some cases the potatoes failed to spring. After some wintry and rainy weather in November, December became a dry and pleasant month, and all the autumnal labours of the field were put into great forwardness.

WEST SIDE OF SCOTLAND.

eorological Observations for 1842, made at Applegarth Manse, Dumfriesshire. By the Rev. WM. DUNBAR, D.D.

12' W. Lat. 55° 13' N. Height above the Sea, 180 feet; Distance from the Sea. 10 miles; The height of the Rain-Gauge from the ground, 5 feet. The observations taken at 9 A.M. and 9 P.M.

BAROMETER.

1842. MONTHS.	Atmospheric Pressure, Morning.	Atmospheric Pressure, Evening.	Mean of Morning and Evening.	Red. to 32° Fahr., and corrected to sea-level.	Mean Daily Range.	Mean Nightly Range.	Mean Range of 24 hours.	Monthly Extremes. Highest.	Lowest.	Geatest range in 24 hours.	Least range in 24 hours.
January,	29.804	29.816	29.810	29.975	0.117	0.128	0.245	30.370	28.770	0.890	0.030
February,	29.681	29.671	29.676	29.821	0.097	0.105	0.202	30.300	28.870	0.580	0.030
March,	29.584	29.549	29.707	29.848	0.167	0.156	0.323	30.180	28.970	1.120	0.060
April,	30.373	30.639	30.206	30.383	0.055	0.044	0.099	30.280	29.110	0.580	0.010
May,	29.442	29.440	29.441	29.546	0.096	0.091	0.187	30.380	28.770	0.740	0.020
June,	29.910	29.866	29.888	29.972	0.092	0.075	0.167	30.280	29.150	0.630	0.060
July,	29.771	29.812	29.791	29.877	0.100	0.090	0.190	30.240	29.150	0.650	0.020
August,	29.896	29.888	29.892	2.9971	0.060	0.082	0.142	30.280	29.520	0.370	0.030
September,	29.771	29.812	29.791	29.880	0.073	0.082	0.155	30.330	29.240	0.460	0.050
October,	29.836	29.697	29.766	29.891	0.090	0.106	0.196	30.410	23.700	0.800	0.020
November,	29.441	29.549	29.545	29.684	0.105	0.162	0.267	30.300	28.630	0.730	0.050
December,	29.755	29.769	29.762	29.897	0.122	0.119	0.241	30.300	28.960	0.620	0.020
Means,	29.772	29.742	29.757	29.896	0.097	0.103	0.100				
1841,	29.633	29.632	29.632	29.757	0.110	0.13	0.223				

THERMOMETER.

MONTHS.	Mean of greatest Heat.	Mean of greatest Cold.	Mean Temp. of Morning.	Mean Temp. of Evening	Mean of Extremes.	Mean of Morning and Evening.	Mean of both.	Mean range of 24 hours.	Monthly Extremes. Highest.	Lowest.	Greatest range of 24 hours.	Least range of 24 hours.	Highest at 9 A.M.	Lowest at 9 A.M.	Highest at 9 P.M.	Lowest at 9 P.M.	Temp. of Spring Water.
Jan.	37.58	29.25	32.03	33.16	33.41	32.59	33.00	8.30	52.00	13.50	26.00	1.50	42.50	13.50	44.00	15.00	48.8
Feb.	39.30	34.90	37.60	39.10	37.10	38.30	37.70	9.50	48.00	26.00	14.50	1.00	44.50	27.00	45.00	30.50	44.3
March	47.60	36.00	41.20	41.30	41.80	41.20	41.52	11.60	57.00	30.50	24.00	5.00	47.50	35.50	48.50	34.00	42.0
April,	54.90	35.70	46.60	46.30	45.30	46.40	45.80	22.70	69.00	29.50	35.00	9.50	59.50	37.50	54.50	35.00	47.0
May,	62.40	44.10	53.30	51.70	53.25	52.50	52.87	16.70	72.00	34.00	29.50	6.50	63.00	45.00	62.00	44.50	46.8
June,	67.00	48.90	59.50	58.80	57.90	59.10	58.50	18.30	78.50	38.00	29.50	8.00	70.00	52.00	66.00	50.00	50.3
July,	64.70	48.70	57.70	56.70	56.70	57.20	56.95	15.70	73.50	41.00	30.50	4.50	66.00	49.50	62.00	52.00	50.0
August,	67.51	52.48	60.25	59.51	59.99	59.88	59.93	15.10	79.00	38.00	26.00	6.00	69.50	52.00	69.00	51.00	50.7
Sept.	61.57	47.73	54.38	54.61	54.65	54.49	54.57	13.70	66.50	41.00	24.50	5.50	61.00	49.00	63.50	48.00	51.6
Oct.	53.20	37.20	43.20	44.20	45.20	43.70	44.45	16.00	65.00	26.50	27.00	7.00	54.50	28.50	53.00	31.00	49.6
Nov.	45.80	36.50	40.00	40.70	41.15	40.35	40.75	9.30	55.00	27.50	16.50	3.50	50.00	31.00	47.00	32.00	47.5
Dec.	50.40	42.20	44.70	46.90	46.30	45.80	46.05	8.50	56.50	30.50	19.50	11.50	52.00	31.50	55.00	35.00	47.6

WINDS—THEIR DIRECTION AND FORCE, AND WEATHER, STATED IN THE NUMBER OF DAYS IN WHICH EACH PREVAILED.

MONTHS.	N.	N.N.E.	N.E.	E.N.E.	E.	E.S.E.	S.E.	S.S.E.	S.	S.S.W.	S.W.	W.S.W.	W.	W.N.W.	N.W.	N.N.W.	Calm.	Mod.	Brisk.	St.Breeze or Boist.	Stormy.	Sun Shone.	Rain Fell.	Snow or Hail.	Frost.	Thunder.	Rain in Inches.
January,	—	—	—	1	11½	—	6	2	2½	—	1	1	1	1	3	1	19	6	1	4	1	16	7	8	21	—	2.26
February,	—	1	—	1	3	2	—	—	4	7	4	1	5	—	—	—	5	12	2	6	3	24	15	1	7	—	2.69
March,	2	—	—	—	—	—	—	3½	1	1	11	1	7½	2	2	—	4	6	2	16	3	23	18	7	4	—	2.74
April,	1	½	5½	3	7	1	—	—	2	—	3	2	1½	—	2	1½	14	9	1	6	—	30	1	H 1	9	—	0.13
May,	—	—	1	—	3	1	5	4½	5	—	4½	4	1½	—	1½	—	7	14	2	8	—	29	12	H 1	—	2	2.58
June,	—	4½	—	—	1½	2	2	1	2½	1	7	1	1	3	3½	—	7	8	9	6	—	28	15	—	—	2	2.22
July,	—	1	2	—	4	—	1	1	4	—	1	2	9	1	3	2	13	8	6	4	—	30	12	—	—	1	3.55
August,	1	1	1½	1	1	—	1	—	4	2	9	—	5½	1	1	2	14	12	4	1	—	29	13	—	—	2	1.35
September,	1	—	7	2	—	1	1	—	2	2	7	—	6	1	—	—	13	6	4	7	—	26	10	—	—	2	1.57
October,	3	—	1	—	—	1	2	1	1	—	4	4	3	6	3	2	12	5	9	5	—	28	4	3	4	—	1.05
November,	1	2	3	1	9	4	1	2	2	—	1	3	—	1	—	—	14	8	1	5	2	24	15	1	7	—	2.77
December,	—	—	—	—	2	3	—	1	11	1	7½	1	3½	—	—	1	4	6½	4	12½	4	19	25	—	3	—	2.91
Total,	9	10	21	9	42	15	19	16	41	14	60	20	44½	16	19	9½	126	100½	45	80½	13	306	147	22	55	9	25.82
1841,	8	6	36	7	36	10½	30	8	27	17	79½	10½	37½	11½	26	13½	97½	118	54½	85	3	295	196	22	56	18	39.80

REMARKS.

The maximum of atmospherical pressure was on the 9th of October, when the Mercury stood at 30.410 inches. The minimum on the 25th October, viz. 28.630. The oscillations of the Mercury less frequent by more than a half than in 1841; the mean daily range of the latter being 0.223 of an inch, while those of the former have been only 0.100.

The maximum of temperature was on the 17th of August, viz. 79 degrees. In 1841, it was 81 degrees. The minimum was on the 16th of January, viz. 13½ degrees. In 1841, it was 1 degree. The mean of extremes exceeds the mean of Morning and Evening by only one-tenth of a degree. The mean temperature of 1842 exceeds that of 1841 by 1¾ degree, and that of the last 20 years by nearly 1½ degree. The high temperature of the month of December deserves to be remarked, being nearly 10 degrees above the mean of December during the last 20 years, and 7 degrees higher than the same month in 1841.

The quantity of Rain fallen in 1842, viz. 25.82 inches, is short of that of 1841 by no less than 14 inches; and of the mean of the last 15 years by nearly 9½ inches. The number of rainy days is one-fourth fewer, the number of stormy days four-fold greater. The driest month in the year, as in 1841, was April, the wettest, July. The Winds from the South a third more frequent, and those from the S.E. less so than in 1841. The Wind from N. to E. blew for 49 days; from E. to S. 92 days; from S. to W. 135 days; and from W. to N. 89 days.

Meteorological Table for 1843.

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Kept at Rutland Square, Edinburgh, by Mr R. D. Paul.

Table I.—January.

1843.	Ther. Max.	Ther. Min.	Ther. Mean.	Barom. ½ p. 8, A. M.	Therm. ½ p. 8, A. M.	Bar. 8, P. M.	Ther. 8, P. M.	Rain.	Hail.		Wind.	Met
Jan. 1.	34°	28°	31°	30.09	28°	29.99	31°	...	...	...	N.	
... 2.	32	23	27	29.96	29	30.00	27	...	...	...	N.	
... 3.	44	32	38	29.93	32	29.69	41	...	...	...	S. W.	
... 4.	36	32	34	29.49	32	29.49	33	...	...	...	N. W.	
... 5.	34	30	32	...	33	29.89	32	...	...	...	N. W.	
... 6.	43	34	38	29.85	35	29.65	36	...	...	...	S. W.	
... 7.	37	28	32	29.40	37	29.13	33	...	...	...	W.	
... 8.	33	24	28	28.92	28	29.10	26	...	...	...	W.	
... 9.	37	32	34	29.29	30	28.66	36	...	...	...	S. W.	
... 10.	33	28	30	28.41	33	28.60	28	...	...	...	N. W.	
... 11.	33	24	28	28.71	29	28.69	28	...	...	...	N. W.	Lunar
... 12.	30	26	28	28.71	28	28.76	26	...	...	...	N. W.	Solar
... 13.	35	32	33	28.32	31	28.23	32	...	...	...	S. E.	
... 14.	32	23	27	28.49	32	28.55	24	...	...	...	N. W.	
... 15.	30	26	28	28.62	27	28.81	26	...	...	...	N.	
... 16.	37	30	33	29.38	36	28.81	31	...	...	...	N.	
... 17.	43	40	41	29.78	38	29.48	43	...	...	...	S. W.	
... 18.	46	44	45	29.96	44	30.11	46	...	...	...	S. W.	
... 19.	46	40	43	30.21	44	30.20	43	...	...	...	S. W.	
... 20.	43	32	37	30.12	42	29.99	35	...	...	...	S. W.	
... 21.	38	32	35	29.81	34	29.71	37	...	...	...	S. E.	
... 22.	45	40	42	29.70	41	29.71	45	...	...	...	S. W.	
... 23.	44	42	43	29.68	41	29.39	43	...	...	...	S.	
... 24.	46	42	44	29.36	44	29.42	43	...	...	...	S. W.	
... 25.	45	42	43	29.55	42	29.67	45	...	...	...	W.	
... 26.	47	43	45	29.69	42	29.68	43	...	...	...	W.	
... 27.	51	44	47	29.46	48	29.38	52	...	...	...	W.	
... 28.	47	40	43	29.21	44	29.50	40	...	...	...	W.	Auror
... 29.	50	40	45	29.45	41	29.30	48	...	...	...	W.	
... 30	44	39	41	29.13	40	29.44	40	...	...	...	N. W.	
... 31.	49	36	42	29.43	44	29.15	48	...	...	...	S. W.	
Means,	40.12	33.80	36.67	29.403	36.41	29.357	36.80	13	7	2		

RESULTS.

BAROMETER.		THERMOMETER.	
Highest, . . .	30.21	Highest, . . .	51°
Lowest, . . .	28.23	Lowest, . . .	23°
Mean, . . .	29.380	Mean, . . .	36.67

WINDS.

W. 7; N.W. 7; N. 4; N.E. 0; E. 0; S.E. 2; S. 1; S.W. 10.

Memoranda.—*January* 1. 2. fine. 3. Cloudy; windy P.M.; thermometer 4 P.M. 37°. 5. Windy P.M. 6. Morning hazy. 7. Stormy at intervals, during day, with rain and sleet; snow 7 P.M.; night stormy. 8. Heavy snow; windy. 9. Snow A.M.; heavy gale after 1 P.M.; snow again, during night. 10. Fine. 11. Fine; hazy; large lunar halo half-past 7 P.M. 12. Hazy; a colourless ring round the sun all day; 8 P.M. ground thickly covered with hoar frost; barometer commenced to sink 11 P.M., the wind at the same time rose from S.E., and an hour or two afterwards, increased to the most violent storm, probably ever recollected, and, at the same time, considering the short time it lasted, was productive of immense loss both of life and property. 13. Seven A.M. stormy, snow occasionally during day, but calm; sleet 10 P.M. 14. 15. Frosty. 16. Barometer rose very rapidly during last night, followed by a thaw. 17. 18. 19. Cloudy. 20. Cloudy; thermometer at 5 P.M. 39°. 21. Fine; thermometer 5 P.M. 32°, but by 10 o'clock P.M. had risen to 38°; night cloudy. 22. Cloudy. 23. Cloudy; stormy at 8 P.M. 24. Windy; rain 9 P.M. 25. Windy; night stormy. 26. Ditto; temperature; 11 P.M. 47°. 27. 28. Stormy. 29. The same; barometer again began to sink at 9 P.M.; wind much higher, with heavy and constant rain. 30. 31. Stormy; nights of both days especially so.

In *London* the storm of the 13th January last was severely felt. About three o'clock A.M. a sharp wind sprang up from south, southwest, and shortly before four o'clock a heavy rain began, which continued until daybreak. About nine o'clock there was a heavy fall of hail; and as the forenoon advanced, the wind increased in violence, until between twelve and one o'clock, it blew a perfect hurricane from the southwest, which lasted for nearly an hour.

Liverpool, Jan. 14.—During the whole of yesterday the falling of the barometer gave unerring symptoms of the approach of a severe storm. The gale increased as the night advanced, and from twelve until five this morning, a hurricane raged, hardly less fierce, but fortunately less destructive as regards life and property, than the memorable one of the 7th January 1839. At noon, on the 14th, barometer at 28.80, having fallen from 28.85, at which point it stood at 9 A.M.

The great and long-continued depression of the barometer, during this month, came to a crisis on the 16th instant. In the morning, it again began to sink after a sudden rise the night before, the wind having veered to north. In the evening of the 16th instant, the barometer was again below 29 inches, and towards midnight the wind went to S.W., when the frost went entirely off, the temperature of the atmosphere becoming extremely mild, without frost even by night; but to make up for this, constant gales and rain prevailed, until the morning of the 2d February, when the frost returned, accompanied by snow from N.W. The depression of the barometer on the 13th was very extensive, the storm not only extending throughout the kingdom, but also on the Continent, where it was, in many parts, more destructive than in Britain.

Table II.—February.

1843.	Ther. Max.	Ther. Min.	Ther. Med.	Barom. ½ p. 8 A. M.	Therm. ½ p. 8 A. M.	Bar. 8 P. M.	Ther. 8 P. M.	Rain.	Hail.		Wind.	Meteors.
Feb. 1.	42°	32°	37°	29.38	38°	29.18	38°	...	...	...	W.	Lightning
... 2.	33	30	31	29.08	32	29.11	32	...	...	...	N. W.	Do.
... 3.	32	19	25	28.89	30	29.13	32	...	...	...	N. W.	
... 4.	34	29	31	29.53	32	29.81	29	...	...	...	N.	
... 5.	35	20	27	29.80	31	29.87	27	...	...	...	N.	
... 6.	35	33	30	29.93	22	29.96	33	...	...	...	N. E.	
... 7.	38	37	37	30.10	38	30.15	37	...	...	...	N. E.	
... 8.	38	36	37	30.10	38	30.05	38	...	...	...	N. E.	
... 9.	36	30	33	30.08	36	30.11	32	...	...	...	N. E.	
... 10.	36	34	35	30.09	33	30.08	34	...	...	...	N. E.	
... 11.	38	36	37	30.10	36	30.10	36	...	...	...	N. E.	
... 12.	40	33	36	30.11	38	30.08	35	...	...	...	E.	
... 13.	35	25	30	29.93	33	29.73	32	...	...	...	W.	
... 14.	28	16	22	29.69	27	29.89	21	...	...	...	N.	
... 15.	27	18	22	29.41	24	29.17	23	...	...	...	W.	
... 16.	28	19	23	29.13	23	29.27	24	...	...	...	N.	
... 17.	32	21	26	29.49	23	29.59	28	...	...	...	N.	
... 18.	34	29	31	29.61	25	29.60	31	...	...	...	N E.	
... 19.	37	32	34	29.60	30	29.50	32	...	...	...	E.	
... 20.	38	35	36	29.41	36	29.39	36	...	...	...	E.	
... 21.	37	35	36	29.39	35	29.41	35	...	...	...	E.	
... 22.	39	34	33	29.41	37	29.40	35	...	...	...	E.	
... 23.	38	35	36	29.49	36	29.52	36	...	...	...	E.	
... 24.	39	35	37	29.60	38	29.65	35	...	...	...	E.	
... 25.	38	33	35	29.66	36	29.62	34	...	...	...	E.	
... 26.	36	33	34	29.57	34	29.38	35	...	...	...	E.	
... 27.	36	31	33	29.18	33	29.09	34	...	...	...	S. E.	
... 28.	36	27	31	29.22	32	29.49	33	...	...	...	N. E.	
Means,	35.53	29.53	32.21	29.605	32.30	29.608	32.39	11	4	13		

RESULTS.

BAROMETER.		THERMOMETER.	
Highest, ...	30.15	Highest, ...	42°
Lowest, . . .	28.89	Lowest, ...	16°
Mean, . . .	29.606	Mean, ...	32.21

WINDS.

W. 3; N.W. 2; N. 5; N.E. 8; E. 9; S.E. 1; S. 0; S.W. 0.

Memoranda.—*Meteor.*—Shortly after eight o'clock, on the evening of the 5th, a brilliant meteor passed over a considerable part of the north of the county of Nottingham. Its course was from N.W., and in its direct path it went a little to the east of Grove, near Retford. Its colour was a dark red, and its velocity not less than 50 or 60 miles in a minute. —*Nottingham Journal.*

The Late Gales.—*Feb.* 18.—During the last six weeks, the sacrifice of life and property at sea has been without parallel in the history of our mercantile affairs. Upon reference to Lloyd's books, it appears that the total number of vessels wrecked during the storm of the 13th January, was 180, and the number of persons lost, 453. On the coast of England 154 vessels, and 190 lives. On the coast of Scotland, 17, and 30 lives. On the coast of Ireland 5, and 104 lives, and on the coast of France 4 vessels, and 100 lives. The value of the vessels and cargoes have been roughly estimated at L.585,000, viz. vessels at L.405,000, and cargoes at L.180,000.

Mean Results of Meteorological Observations at Ancaster, Canada West, for the Year 1842. By Wm. Craigie, Esq., Surgeon.

1842.	THERMOMETER.					BAROMETER.			RAIN.			
	Mean 9 A.M.	Mean 9 P. .	Mean of both.	Highest of the Month.	Lowest in the Month.	Mean Height in Inches.	Highest.	Lowest.	Depth in Inches.	Number of Fair Days.	Number of Rainy Days.	Days with slight Showers
January, ...	30°.45	32°.10	31°.275	53°	12°	28.922	29.46	28.50	2.45	21	3	7
February,...	30.82	32.00	31.41	56	8	28.947	29.37	28.40	2.2	21	4	3
March, ...	39.39	40.87	40.13	70	15	29.062	29.45	28.55	2.48	20	4	7
April, ...	46.20	47.8	47.	87	33	29.0325	29.39	28.55	3.5	21	6	3
May, ...	53.42	53.50	53.46	76	32	29.049	29.43	28.70	0.9	25	1	5
June,.........	60.9	58.8	59.85	81	32	29.038	29.36	28.74	3.6	16	6	8
July, ...	67.84	66.67	67.26	89	50	29.135	29.40	28.83	4.8	24	3	4
August,......	66.22	66.6	66.41	84	50	29.173	29.40	28.88	2.7	21	3	7
September,	57.7	57.5	57.6	83	32	29.109	29.33	28.70	3.75	21	5	4
October, ...	49.13	49.71	49.42	69	33	29.077	29.35	28.64	1.75	23	3	5
November,	35.23	36.2	35.72	63	12	29.016	29.50	28.28	4.05	16	6	8
December,	29.06	29.5	29.28	53	10	29.038	29.48	28.52	4.5	21	5	5
Means, ...	17.197	47.6	47.40	...	...	29.049	...	...	36.68	250	49	66

YEARS.

Mean Temperature of	
1835,............	45.318
1836,............	43.405
1837,............	44.237
1838,............	45.205
1839,............	47.618
1840,............	47.807
1841,............	47.523
1842,............	47.40

Mean of 8 years, 46.064

NOTE.—The quantity of rain is only an approximation, the gauge which is small, and not in a very good situation, having been fitted up for the purpose of ascertaining how far it was practicable to melt the snow during the low temperature of the winter months, and in this it has succeeded beyond expectation.

Proceedings of the Royal Society of Edinburgh.

(Continued from last Number, p. 176.)

1842, *December* 5.—Sir T. Makdougall Brisbane, Bart., President, in the Chair.

1. On the Construction of a New Music Hall. By Sir George S. Mackenzie, Bart.

December 19.—The Right Honourable Lord Greenock, Vice-President, in the Chair.

1. Letter on Terrestrial Magnetism, addressed to the Secretary. By Professor Hansteen of Christiania.
2. Notice of the occurrence in Scotland of the *Tetrao medius*, shewing that supposed species to be a hybrid. By James Wilson, Esq.

There exists in several northern continental countries a peculiar kind of grouse, called by foreign naturalists *Tetrao medius*, on account of its exhibiting, as it were, a combination of the characters of the wood-grouse or capercailzie on the one hand, and of the black-cock on the other. It is never found except in countries inhabited by the two species last named; and as it presents a union of their characters, several naturalists have inferred that it is not itself a distinct kind, but a hybrid, resulting from the casual intercourse of the other two. But most naturalists have maintained that it is a distinct species, chiefly upon the principle, that, in the wild state, birds of different species do not intermingle sexually with each other. Mr Wilson, however, having discovered that, in certain districts of Scotland into which Lord Breadalbane has lately introduced the capercailzie, and in which the black-cock previously existed in abundance, this so-called intermediate grouse has also now made its appearance, he draws the conclusion, that it is not a distinct species, but a hybrid or mule. "It had not been previously known in Scotland, at least in our times,—it has not been introduced by any one from abroad,—and yet here we now find it in the very districts inhabited by the other two." Mr Wilson exhibited a specimen recently killed on the estate of Dunira, and shewed its entire agreement with the foreign *T. medius*, by comparing it with a specimen from Norway.

3. On the Coloration of the Blood. By the late Daniel Ellis, Esq., F.R.S.E. Communicated by Dr Christison.

1843, *January* 9.—Dr Abercrombie, Vice-President, in the Chair.

1. On the Growth of the Salmon. By Mr Andrew Young, Invershin, Sutherlandshire. Communicated by James Wilson, Esq.

Mr Young has here taken up the subject of the salmon's growth where it was necessarily left off by Mr Shaw. So far as the earliest or fresh-water state of the fish is concerned, he entirely agrees with the observer just named. He then states the various opinions which prevail regarding the more or less rapid growth of smolts and grilse, and shews by tabular lists (the result of frequently repeated experiments), that the increase in their dimensions is extraordinary so soon as they descend into the salt water. So far back as the months of April and May 1837, he marked a number of descending smolts, by making a peculiar perforation in the caudal fin, by means of small nipping irons constructed for the purpose. He re-captured a considerable number of them ascending the rivers as grilse, in the course of the ensuing months of June and July, and weighing several pounds each, more or less according to the difference in the length of their sojourn in the sea. Again, in April and May of 1842, he marked a number of descending smolts, by clipping off the little adipose fin upon the back. In June and July he caught several of them returning up the river, and bearing his peculiar mark,—the adipose fin being absent. Two of these specimens were exhibited to the Society. One marked in April, and re-captured on the 25th of July, weighed 7 lb.; the other, marked in May and recaptured on the 30th July, weighed 3½ lb. As the season advances grilse increase in size, those being the largest which abide the longest in the sea. They spawn in the rivers after their first ascent, and before they have become adult salmon.

Mr Young also described various experiments instituted with the view of shewing the transition of grilse into salmon. He marked many small grilse after they had spawned in winter, and were about to re-descend into the sea. He re-captured them in the course of the ensuing summer as finely-formed salmon, ranging in weight from 9 to 14 lb., the difference still depending on the length of their sojourn in the sea. He has tried these experiments for many seasons, but never twice with the same mark. A specimen marked as a grilse of 4 lb. in January 1842, and re-captured as a salmon of 9 lb. in July, was exhibited to the Society. It bore a peculiarly twisted piece of *copper* wire in the upper lobe of the caudal fin.

Those marked and re-taken in 1841 were marked with *brass* wire in the dorsal fin. With these and other precautions Mr Young avoided the possibility of any mistake as to the lapse of time. Both grilse and salmon return uniformly to their native streams; at least it very rarely happens that a fish bearing a particular mark is found except in the river where it was so marked. Salmon in the perfect state as to form and aspect, also increase rapidly in their dimensions on again reaching the sea. A spawned salmon weighing 12 lb. was marked on the 4th of March, and was re-captured on its return from the sea on the 10th of July, weighing 18 lb. Mr Young is of opinion that salmon rather diminish than increase in size during their sojourn in rivers; and he illustrates this and other points of his subject by numerous experiments and observations.

2. On the Geology of Roxburghshire. By David Milne, Esq.

Mr Milne divided his paper into two parts; the first comprehending a description of the leading geological features of the district; the second containing the inferences of a cosmological character, which the facts related in the first part seemed to warrant.

In describing the geology of Roxburghshire, Mr Milne referred, *first*, to the stratified rocks; *secondly*, to the igneous rocks; and, *thirdly*, to the superficial, or (as they have been sometimes termed) the diluvial deposits.

The stratified rocks were stated to consist of the following series, beginning with the oldest, viz.—greywacke, old red sandstone, and the coal measures. As to the long-disputed question regarding the existence of the new-red sandstone formation in this county, Mr Milne, whilst not wishing to affirm absolutely the non-existence of any strata whatever belonging to this epoch, referred to the older formation the great mass of the red sandstones abounding in the district, adding that he had himself seen none which necessarily belonged to a later epoch.

It was stated that no fossils had been found in the greywacke strata, but that in the old red sandstone formation, scales and bones of the *Holoptychius* had been found embedded both in the red and the white coloured strata.

The igneous rocks consist of all the varieties of felspars, basalts, and greenstones, known in other parts of Scotland, the first mentioned of these being the oldest. All these rocks occur in the form of dykes, as well as hills, of which the Eildons and Cheviots are the highest and most extensive.

The superficial deposits consist, beginning with the oldest, of the boulder clay, well known in the Lothians,—of sand and gravels,—and of great blocks or rounded fragments of rocks, all strewed over the surface. It was mentioned, that, whilst the boulder clay was deposited in tumultuous waters (presenting no signs of stratification), the sands and gravels being for the most part stratified, have been deposited by waters not in violent action. The greater number of boulders in Liddesdale consist of grey granite, very similar to that of Criffel, situated between thirty and forty miles to the westward.

In part 2d, the author observed, that the greywacke formation, presenting as they do enormous foldings, in consequence of which the formation is traversed by ridges and valleys, all running east and west by compass, must have been acted on here, as throughout the rest of this part of the island, by a force or system of forces, which acted in a particular direction; and that as hardly any igneous rocks whatever occur, within the limits of this formation, it seemed that the greywacke strata had not been elevated and folded together by igneous action, but more probably in consequence of changes in the form of the earth's nucleus, as suggested by Elie de Beaumont.

The elevation of the greywacke ranges was followed by eruptions of felspathic and a few greenstone rocks, which took place chiefly on the outskirts of that formation; and from the sediment afforded by the wearing down of these rocks, still at the bottom of a sea, the stratified rocks surrounding and partly covering these older rocks were formed. As the heaviest sediment would be deposited first, the sandstones filled with oxide of iron, and now constituting the principal beds of the old red sandstone formation, would girdle the hills of greywacke and older felspathic rocks; whilst the strata of white sandstone, shales, and limestones, being composed of lighter sediment, would be carried farther, and become members of the coal measures situated in Liddesdale, Northumberland, and Berwickshire.

The formation of the whinstone dykes, one of which was described as running in a NW. direction, for about twenty-four miles, was ascribed by the author to the irruption of igneous matter into fissures previously formed in the earth's crust.

The beds of gravel and sand, as well as the boulders, the author thought might all be explained on the supposition, that the district had been covered by the waters of the ocean, when they were deposited. He adduced facts and arguments for the purpose of shewing that certainly none of these deposits could have been formed by glacial action, and that probably submarine currents, or great waves, such as are known to have been produced by submarine eruptions, would be sufficient to account for all the phenomena.

3. On the Property of Transmitting Light, possessed by Charcoal and Plumbago, in fine plates and particles. By John Davy, M.D., &c.

The charcoal of the pith of the elder consists of plates of extraordinary thinness. It was in examining this charcoal, that the author first observed the property which is the subject of his paper. He detected it by means of the microscope, using a high magnifying power. By analogy, he was led to infer that the power of transmitting light must belong to charcoal in general, in all its varieties, when reduced to the state of fine powder or filaments,—an influence which he found confirmed by experiment in a number of different instances, as the charcoal of the pith of the sycamore, of the pith of the rush, the fibre of cotton, flax, &c. He also found it to belong to lamp-black, to cork in very fine powder, to anthracite, and plumbago.

The light transmitted he found to vary in its hues, from almost white, as in the instance of the thinnest plates of the charcoal of the pith of the elder, to brown and red of various shades, in the instances of lamp-black, anthracite, and plumbago.

He considers the property of translucency belonging to charcoal and plumbago, in their finely divided state, as favourable to the opinion now commonly received, that these substances and diamond owe their marked peculiarities not to difference of chemical mixture, but of mechanical structure. Incidentally, he notices the specific gravities of these substances,—stating, as the result of his own experiments, that the specific gravity of charcoal, cork, and anthracite, is about 1.5; and that of plumbago about the same, making allowance for the ferruginous and earthy matter with which the carbon in this mineral is mixed.

In conclusion, he offers the conjecture, that the coloured tints of vapour and fluids in which carbon is suspended, may be connected with the translucency of this substance, and that other bodies, hitherto considered opaque, may be found capable of transmitting light, when examined in a manner similar to that which he has employed.

January 23.—The Very Reverend Principal Lee, V.P., in the Chair.

1. Chemical Observations on the Flowers of the Camellia Japonica, Magnolia grandiflora, and Chrysanthemum Leucanthemum; and on three proximate principles which they contain. Part I. By Dr Hope.

2. On the Law of Visible Position in Single and Binocular Vision, and on the Representation of Solid Figures, by the union of two dissimilar plane figures on the Retinæ. By Sir David Brewster, K.H. Part I.

Proceedings of the Wernerian Natural History Society.

(Continued from last No., p. 177.)

December 10. 1842.—Professor JAMESON, President, in the Chair.

Mr Torrie read Mr Henry Goodsir's account of two new genera of Crustacea, found by him in the Firth of Forth, and to which he has given the names of *Bodotria* and *Alauna* (published in the last No. of this Journal, p. 119); also Dr Traill's description of a new species of Serpent from Demerara, which he has named *Elaps Jamesoni* (published in the last No. of this Journal, p. 53). There was exhibited a very fine specimen of the *Squalus vulpes*, or Fox-Shark, 13 feet long, taken in Largo Bay in August last.

January 28. 1843.—The Right Honourable Lord GREENOCK, V.P., in the Chair.

Dr Neill read a notice regarding the ventriloquistic song of a redbreast, contained in a letter addressed to him by James Heriot, Esq. of Ramornie. Dr Hamilton read a paper communicated to the Society, entitled, The Ancient Chronology of the World, and its application to Geology and the Natural History of Man.

February 25.—Sir WILLIAM NEWBIGGING, V.P. in the Chair.

Mr Torrie read Dr Mathie Hamilton's observations on the Llama, Alpaca, Vicuna, and Guanaco of Peru (published in the present No. of this Journal, p. 285). Mr John Goodsir read a paper by his brother, Henry D. G. Goodsir, Esq., surgeon, describing the *Maidre*, or vast accumulation of minute marine animals which precedes the appearance of a herring shoal, as observed off the Isle of May; and detailing the characters of a new species of *Cetochilus*. Mr Torrie read Dr Mathie Hamilton's remarks on the production, &c. of the Guano of commerce.

March 18.—Professor JAMESON, President, in the Chair.

Mr Torrie read an account of the great explosion at Dover, by Captain Stuart, communicated by Lord Greenock (published in this No. of Journal, p. 337). Dr Traill read his paper on the introduction into Scotland of granite for ornamental purposes by Messrs Macdonald and Leslie, Aberdeen (published in this No. p. 341). Mr Torrie read a communication on the habits and structure of the Tinamus Guianensis by Dr Frazer, late of Demerara. Various Meteorological Tables were laid on the table.

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Variation of Temperature during the Russian Expedition to Khiva.*—It has been stated to the Academy of Sciences by a Russian officer who had accompanied the army to Khiva, that during the expedition, the thermometer fell to − 43° C. (− 45°.4 Fahr.); that for more than three months the mean temperature was between − 17° and − 18° (+ 1°.4 and − 0.4 F.); and that during their return, in the month of June 1840, the thermometer rose to + 46° C. (+ 114°.8 F.) Thus, in the course of a few months, the troops were exposed to a variation of 89 degrees Centigrade, or 160 degrees Fahrenheit.

2. *On the Movement and Structure of the Mer de Glace of Chamouni.*—On the 27th February 1843, Professor Forbes read a memoir before the Royal Society of Edinburgh "On the Motion of the Mer de Glace of Chamouni."

The author detailed in this paper the methods of observation by which he was enabled to ascertain the *daily* and even *hourly* motion of different parts of the glacier.

The following are some of the principal results :—

I. In the particular case of the Mer de Glace, the motion of the higher parts of the glacier are on the whole slower than those of its lower portion, but the motion of the middle region is slower than either.

The following table, the result of observations at a series of ascending stations, will authorize this conclusion.

	Velocity.
Lower part, ...	1.000 0.770
Middle do., ...	0.479
Higher do., ...	0.674

II. The Glacier du Géant moves faster than the Glacier de Lechaud in the proportion of 7 to 6.

III. The centre of the glacier moves faster than the sides. When two glaciers unite, they act as a single one in this respect, just as two united rivers would do.

The author measured the velocities at different places in the breadth of the glacier, and it was found to increase towards the centre. The following are the numerical results, assuming the motion of the ice near the edge as the standard or the unit of reference.

Side.			Centre.
1.000	1.332	1.356	1.367

IV. The difference of motion of the centre and sides of the glacier varies (1) with the season of the year, and (2) at different parts of the length of the glacier.

1. From the observations made, the author concludes, that "the variation of velocity diminished as the season advanced; and that it was proportional to the absolute velocity of the glacier at the same time."

2. The variation of the velocity with the breadth of the glacier is least considerable in the higher parts of the glacier, or near its origin.

V. The motion of the glacier generally varies with the season of the year and the state of the thermometer.

Perhaps the most critical consideration of any for the various theories of glacier motion is the influence of external temperature upon the velocity. It is shewn in this paper, by a direct numerical comparison, and by projected curves, that in nearly every instance the velocity of the glacier, during any period of days, has a reference to the temperature of the same period. If the thermometer fell, the glacier advanced slower, and *vice versa*. It is not, however, to be inferred that at the same external temperature the velocity will always be the same; only at any season, the change will always be in the same *direction*, and governed by the thermometer, though not always the same in amount.

The author also deduced from various indirect considerations, that it is very improbable that the glacier *stands still* in winter. On the contrary, he supposes that though its velocity is less than in summer, it still bears a considerable proportion to it.

On the 20th March 1843, Professor Forbes read a memoir to the Royal Society of Edinburgh, on the structure of glaciers and the cause of their motion.

With reference to his former paper of the 27th February, the author stated that he had received a most satisfactory confirmation of his opinion respecting the motion of glaciers in winter. From observations made by his direction on the Mer de Glace of Chamouni, and in which he places entire confidence, it appears that the ice moved no less than 76 feet between the 12th December 1842 and 17th February 1843, or at the rate of 13½ inches, *per diem*, whilst its mean motion during the summer was 17½ inches.

The author then explained the manner in which he conceives the conoidal structure of glaciers to be due to the varying velocity of different points of their section producing discontinuity by minute fissures, which are infiltrated and ultimately frozen. He had before satisfied himself that the forms of these surfaces are such as the motion of the particles of a viscid fluid, obstructed by the sides and bottom of the canal in which it moves, would engender. But to make this more palpable, he has endeavoured to imitate the motion of a glacier, by causing a plastic fluid of different colours to mould itself by the action of gravity in an inclined bed, and he has thus succeeded in reproducing the forms of the structural surfaces of glaciers so precisely that they cannot be distinguished from the curves which he had drawn as representing the actual phenomena.—*See Edinburgh Philosophical Journal*, Oct. 1842, pages 346, 347.*

Professor Forbes recapitulated the proofs that the glacier moves as a plastic mass, the friction of whose parts is less than their friction upon the surface over which they tend to slide; and he bases his theory upon three classes of facts, which he considers that he has demonstrated. 1. That the glacier moves like a stream, fastest at the centre. 2. That its velocity is immediately governed by the external temperature and the state of infiltration of the ice by water at the time. 3. That the forms which its veined structure assumes are those due to the movement of a semi-solid mass in the manner supposed.

3. *Climate of Malta.*—Many of the remarks which have been made on the Ionian Islands, in relation to climate and seasons, are necessarily applicable to Malta. Situated farther south, its mean annual temperature is higher; its surface being less elevated, its highest hills not exceeding 600 feet; and being farther removed from lofty mountains, and surrounded by a greater expanse of sea, its temperature during the greater part of the year is more equable; and lastly, being nearer to the coast of Africa, it is more liable to be invaded by hot winds, and in summer to experience excessive degrees of heat.

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* Our readers are requested to correct a typographical error at line 6, p. 352, vol. xxxiii, viz., for *annular* rings, read *annual* rings.—Edit.

As regards temperature, in considering the climate of Malta, it is necessary to distinguish between the town and the country, the circumstances of the two being in many respects peculiar, and occasioning a marked difference in the results of the thermometrical observations. The town of Valetta, by its massive buildings and comparatively narrow streets, is well fitted to equalize temperature. The country, on the contrary, being almost entirely destitute of wood, its surface rocky, its soil scanty, is better adapted to radiate heat. This distinction is commonly neglected, and, in consequence, the observations which have been made in the city have been applied to the whole island; and an exaggerated idea has been formed of the equability of the temperature of Malta, and especially during the heats of summer.*

4. *Ignis Fatuus (Will-with-a-Wisp, Jack-with-a-Lantern, Spunkie) observed near Bologna.*—In the Annali di Fisica, &c. (vol. iii. p. 36), there is an interesting notice respecting this phenomenon by Dr Quirino Barillic Filepauti, from which we think it proper to make the following extract:—

"The painter Onofrio Zanotti assured me, that one evening, as he was walking with some one in the street Lungo-Reno, he saw, near the house of Professor Santini, globes of fire, in the form of flames, issuing from between the paving-stones of the street, and even among his feet. They rose upwards and disappeared; he even felt their heat when they passed near him. According to the information I have collected from many individuals, I have ascertained that St Elmo's fire is often seen in the neighbourhood of the town, and I have learned in what places it appears most frequently. I have therefore gone in the evening, sometimes to one place, sometimes to another, and continued my observations for many days, both during a clear and cloudy sky. I took up my station chiefly at the entrance to the cemetery, because I was assured that it was there in particular where it appeared, although, in fact, I did not notice one at this point. These researches were undertaken in the autumn, when, according to the general opinion, this luminous phenomenon shews itself more frequently than at any other season, perhaps on account of the rapid changes of the atmospheric pressure, which allow the gases enclosed in the earth to escape more easily, by favouring their natural elasticity.

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* Mountains and valleys, the former considerably below the region of perpetual snow, the latter moderately open and exposed to sunshine, appear to have an effect in equalizing temperature somewhat similar to that of massive buildings in towns and narrow streets. In travelling on the continent late in autumn, and in the depth of winter, in passing from a low plain country, as from France into Savoy, or from Lower into Upper Austria, I have been struck with surprise at the mildness of the air of the mountain valleys compared with the cold experienced in the lower and open country. But, on reflection, is not the difference such as might be expected, considering the causes in operation which have an effect on atmospheric temperature, and especially those connected with the radiation of heat? The damp mountain forests, in absorbing and giving out heat, may act like mountain lakes and streams. The rocks on the mountain sides, besides absorbing and giving out heat, must throw back heat which they receive from the valleys. In the economy of nature, the circumstances alluded to seem to be a beautiful provision for softening the severity of winter, and rendering habitable regions which the imagination is disposed to conceive the seat of storms and inclemency during the winter season.—*Dr Davy on the Ionian Islands and Malta*, vol. i. p. 257.

I perceived only three of these lights, but on different nights. The first was one of those which issue from the ground, rise to a certain height, and then suddenly become extinguished. I can say nothing more respecting these than that they rise rapidly in a vertical line to a height of three or four metres, and then become extinct with a slight detonation. The second moved in a horizontal direction, and I could not long follow it. The wind carried it to the banks of the river Idice, where it disappeared. With regard to the third, which afforded me the opportunity of making the experiments I wished, I must enter into more circumstantial details.

A place fruitful of ignes fatui is the parish of San-Donino, particularly in the neighbourhood of the small church of Ascension, about two miles from Bologna, and especially quite close to a pool, in a rivulet where, three years ago, three sacrificial vessels of fine Roman workmanship were found. On many successive nights I have repaired to this spot, but in vain. However, one evening in October, which was succeeded by an aurora borealis and rain, I entered the house of a peasant on the field where the pool occurs. Shortly after, I opened the window, which overlooks the place where the phenomenon most commonly shews itself. About 11 o'clock I saw the light appear which I was desirous to observe; and I instantly seized the stick which I always kept ready for the purpose, and which had some flax attached to its extremity, and speedily repaired to the spot indicated. When I was not more than about twenty feet from the light, I stopped a moment to observe it. It had the form and colour of an ordinary flame, with a slight discharge of smoke. Its thickness was about a decimetre; and it was moving slowly in a direction from south to north. When I approached nearer it changed its direction, retired from me, and began to rise upwards. I hurried forward with my stick, and thrust it into the flame, which kindled the flax. Soon after, the Jack-o'-lantern became extinct at a height of about two or three feet above the stature of a man. It soon after reappeared of smaller size (for I was led to believe that it was the same), on another pool placed at a little distance. I ran immediately towards it, but in vain, as it vanished in a few seconds. I saw no others that night. The remains of the flax had not that garlick-like smell peculiar to phosphorus, but a faint peculiar odour which I cannot define, and which appeared to me to be rather of a sulphureous and ammoniacal nature.*

GEOLOGY.

5. *Geological Chronometer.*—The *Athenæum* gives an abstract of a paper, read by Mr Lyell to the Geological Society, which affords some data for guessing at the period when the Mastodon lived, the gigantic quadruped whose bones are found in the soil in various parts of North America. Near Goat Island, which is close to the Falls of Niagara, and at the Whirlpool, which is four miles further down, Mr Lyell found a fluviatile deposit, 40 feet thick at the latter locality, consisting of beds of sand, and containing many recent shells, with remains of the Mastodon. When the deposit was formed by the river, its waters must have been 300 feet higher than at present. It follows, that the deep channel from the Whirlpool to Goat's Island was then uncut, and that the Falls were below the Whirlpool. Hence, it appears, that since the bones of the Mastodon were deposited in these beds, the Falls have receded (according to maps in our possession) four miles, and possibly much more, for when the deposit was formed, the Falls may have been, not at the Whirlpool, but some miles below it. According to an estimate made some years ago, the Falls recede (by undermining the rock) about a yard per annum, but Mr Lyell assigns a foot as the more probable amount; and as they have receded in this case four miles, or 20,000 feet, we may infer that 20,000 years have elapsed since the bones were deposited in the fluviatile sediment, and since the animal lived. If the estimated rate of recession is accurate, the time cannot be less than this, but it *may be* more. The result, though wanting precision, is not without its value; and there is little doubt that by the aid of such natural Chronometers as Niagara Falls, and other means, we shall by and by be able to measure by centuries geological periods of

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* L'Institut, No. 471, 5th January 1843, p. 8.

in the Oural Mountains, weighed about 10 kilogrammes (24 Russian pounds and 69 zolotnies = 10.117 kil.), or upwards of 22 lb. English;* and it is that of which there is a plaster model in the Museum of Natural History at Paris. On the 7th November last, however, there was found in the same mountains a mass of native gold, weighing more than three times as much, viz. 36.025 kil. (2 pouds, 7 Russian pounds, and 92 zolotnies) = about 80 pounds English. The mines of Zarevo-Nicolaefsy and of Zarevo-Alexandrofsy, situated in the alluvial auriferous deposits of Miass, on the Asiatic side of the southern portion of the Oural, have already afforded more than 6500 kilogrammes of gold. It was in this alluvium that, in 1836, the large mass of 10 kil., and several others, of from 4 to 6½ kil. were found at a depth of a few centimetres† under the surface. Subsequently to the year 1837, the mines of Nicolaefsy and of Alexandrofsy seeming exhausted, new explorations were made in the neighbourhood, and especially along the river Tachnou-Targanna. Great success attended the search for gold in that marshy plain, and the whole valley had been searched except that part of it occupied by the building in which the washing operations were carried on. In 1842 it was resolved to remove the houses, whereupon sands were met with of immense richness, and lastly there was discovered under the very corner of a building, and at a depth of three yards, the enormous mass of gold weighing 36 kilogrammes. This mass is already placed in the collection of the *Corps des Mines* at St Petersburgh. According to the information given by M. de Humboldt, in the third volume of his *Examen critique de la Géographie du nouveau Continent,* the mass of gold found in the Oural in 1826 was inferior in weight to that discovered in 1502 in the alluvium of the Island of Haïti, and inferior also to that found in 1821 in the United States, in the county of Cavarras, and described by M. Zoehler, a pupil of the Freyberg School of Mines. The mass found at Miass, fifteen years ago, weighs 10.117 kil.; that of Cavarras 12.600 kil.; that of Haïti 14 to 15 kil.; but the mass of gold found in November 1842 in beds of alluvium reposing on diorite is more than twice the weight of the largest of these, as it weighs no less than 36 kilogrammes. Such has been the prodigious increase of the quantity of gold obtained by washing in Russia, and especially in Siberia, to the east of the southern chain of

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* A French kilogramme = 2.20548 lb. avoirdupois.—Edit.

† A centimetre = 0.393708 inches.—Edit.

then seems to have been viewed as one of the precious metals, as Achilles proposed a ball of iron as one of the prizes to be awarded to the victor of the games instituted in honour of Patroclus;—offerings of iron implements were also made to the gods.

With regard to the early use of bronze in preference to iron, we cannot forbear transcribing some remarks from Robertson's History of North America:—"Gold, silver, and copper, are found, in their perfect state, in the clefts of rocks, in the sides of mountains, or the channels of rivers. They were accordingly first known and first applied to use. But iron, the most serviceable of all metals, and to which man is most indebted, is never discovered in its perfect form; its gross and stubborn ore must feel twice the force of fire, and go through two laborious processes before it becomes fit for use. Man was long acquainted with other metals before he acquired the art of fabricating iron."

Several small articles of iron have been found in Egyptian tombs; but though acquainted with it, they do not appear to have applied it to any practically useful purpose.

In the British Museum are several chisels, saws, and other tools of bronze; and the author has a fish-hook of the same material, found in a tomb, and also several pins of the latest modern improvement, namely, with solid heads. A small bronze knife, found at Thebes, was highly elastic, and the edge, after being buried at least 2000 years, so perfect, that it was used for a penknife for several months after its exhumation.—*The London Journal and Repertory of Arts, Sciences, and Manufactures*, No. cxxv. No. 296.

10. *On the Production of the Guano of Commerce.*—The Moro of Arica is situated close to the town, on the south, and is a bold promontory projecting towards the sea, its base being washed by the surf of the Pacific Ocean, and its summit being about 600 feet above it. This Moro presents a precipice nearly perpendicular, with numerous cliffs or ledges, which during ages have been occupied by myriads of sea-fowl, called Garza by the Spaniards, but better known by the Peruvian name, *Guano*,—a term which is also used by the Indians for the dung of these birds. The front of the Moro of Arica is a most conspicuous and important object to mariners, who wish to call there; for when vessels coming from the south, or windward, as it is there called, are allowed by those on board to pass the port, the space gone over in a few hours may be such as to require several

days to beat up again to the roadstead. But in consequence of Guanos nestling on the face of the Moro, it has a white appearance, from the accumulation of their droppings, which, when recent and dry, as it always is in that locality, is of a grey-white colour, and serves both as a beacon to the navigator who approaches the place, and also as a magnificent object, when seen under the rays of the setting sun. The dung of the guano has been used for manure by the Peruvians, from time immemorial, and is highly prized by them, on account of its fertilizing properties, which are very great. I have seen some of these inoffensive beings, who had come several hundred miles, having traversed ravines and tracks over all but impassable mountains, each one with his donkey or llama, for a quintal of guano, with which he had to march back again, trudging on foot, and often rejoicing over his odorous cargo. The guanos were still to be seen in vast numbers on the Moro of Arica, during my first residence there in 1826, but not in such abundance as they were a few years prior to that period; for during the war for independence, Arica was several times attacked, both by sea and land, when the cannonading had the effect of scaring the guanos from their haunts on the Moro. Since 1826, Arica has been much frequented by foreigners, some of whom often fired at, and otherwise annoyed these birds, which now have all but totally abandoned that part of the Peruvian coast. The guanos have hitherto existed on the coast of the Peru, in numbers which would appear incredible, except to those persons who have seen them. The greatest mass of guanos I ever saw was in 1836, at the Chincha Isles, which are only barren [rocks in the Pacific Ocean, off Pisco, and about 100 miles south from Callio. I saw the birds through a glass from on board a vessel under easy sail, when the rock appeared to be a living mass; for the guanos seemed to be contending among themselves for a resting-place. They live on fish, and are expert fishers, for which they are beautifully formed by nature. The bill is three or four inches long, according to the age or size of the bird, and it is about one inch broad at the extremity, much curved, and altogether well adapted for hooking up the food, which rarely escapes. The quantity of guano manure accumulated on the Peruvian coast must have been very great, and may be estimated thus: Allowing the average number of these birds to be one million, which I consider is much within bounds, and that each guano has one ounce droppings per day, we shall have not less than above thirty tons, and deducting one-half of the above supposed quantity, for evaporation, and other casualties, there will still be above fifteen tons of this valuable substance produced every day. From what has been observed as to the habits and numbers of the guano, their frequenting promontories, declivities, and insulated rocks, it follows, that their excrements in certain localities must have accumulated to such an extent, as might induce those persons who may not have considered the subject, to expect that the guano is to be had in unlimited quantity; but for obvious reasons, that must be a fallacious expectation.—*Communicated by Dr Mathie Hamilton, late of Peru.*

11. *Visit of Columbus to Iceland, in* 1477, *and his Conversations there with learned men.*—Karl Wilhelmi, in his recently published work on the Northmen, has the following curious passage regarding Columbus:—" The most remarkable, and the most peculiar state founded by the Northmen, was that in Iceland, as well on account of the particular northern mode of life which was there freely developed to its fullest extent, and which preserved, unimpaired for centuries, its laws, language, eloquence, music, and poetry, as of the discovery of America, which was made from that country five hundred years before Columbus. That immortal Genoese himself sailed from England, in a ship from Bristol, in the year 1477, and visited the island of Iceland, where he was confirmed in his conviction of the existence of land in the West, by the conversations he carried on in the Latin language, with the Icelandic priests, and other learned men." * In regard to this subject, Washington Irving, in his Life of Columbus, vol. i. p. 69, says,—" While the design of attempting the discovery in the West was maturing in the mind of Columbus, he made a voyage to the north of Europe. Of this we have no other memorial than the following passage, extracted by his son from one of his letters:—' In the year 1477, in February, I navigated one hundred leagues beyond Thule, the southern part of which is seventy-three degrees distant from the Equator, and not sixty-three, as some pretend; neither is it situated within the line which includes the west of Ptolemy, but is much more westerly. The

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* *Island, Hvitramannaland, Grönland, und Vinland, oder der Norrmänner Leben auf Island und Grönland, und deren Fahrten nach Amerika schon über* 500 *Jahre vor Columbus.* Heidelberg, 1842.

English, principally those of Bristol, go with their merchandize to this island, which is as large as England. When I was there the sea was not frozen, and the tides were so great, as to rise and fall twenty six fathoms.'* The island thus mentioned as Thule, is generally supposed to have been Iceland, which is far to the west of the Ultima Thule of the ancients, as laid down in the map of Ptolemy. Nothing more is known of this voyage, in which we discern indications of that ardent and impatient desire to break away from the limits of the Old World, and launch into the unknown regions of the ocean."

12. *Ethnological Society.*—We are happy to announce the formation in London of a society, which promises much for an important but hitherto much neglected branch of knowledge. The following was communicated to us by the Secretary:—

"It is submitted, that among the numerous Literary and Scientific Societies established in the British Metropolis, one is still wanting to complete the circle of Scientific Institutions, whose sole object should be the promotion and diffusion of the most important and interesting branch of knowledge, that of man, viz. ETHNOLOGY.

—"That a new and useful Society might therefore be formed, under the name of 'The Ethnological Society.'

—"That the interest excited by this department of science is increasingly felt;—that its advantages are of the first importance to mankind in general, and paramount to the welfare of a maritime nation like Great Britain, with its numerous and extensive Colonies and Foreign Possessions.

—"That although there is a great amount of Ethnological information existing in Great Britain, yet it is so scattered and dispersed, either in large books that are not generally accessible, or in the bureaux of the public departments, or in the possession of private individuals, as to be nearly unavailable to the public.

"The objects, then, of such a Society as is now suggested would be—

"1. To collect, register, and digest, and to print for the use of the members and the public at large, in a cheap form, and at certain intervals, such new, interesting and useful facts as the Society may have in its possession, and may from time to time acquire.

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* Hist. del Almirante, c. 4.

"2. To accumulate gradually a Museum illustrative of the varieties of mankind, and of the arts of uncivilized life—a Library of the best books on Ethnology—a selection of the best Voyages and Travels—a complete collection of Dictionaries and Grammars bearing upon the subject—as well as all such documents and materials as may convey the best information to persons intending to visit Foreign Countries: it being of the greatest utility to those who are about to travel, to be aware of what has been already done, and what is still wanting, in the countries which they may intend to visit.

"3. To render pecuniary assistance, when the funds will permit, to such Travellers as may require it, in order to facilitate this particular branch of their research.

"4. To correspond with similar Societies that may be established in different parts of the world, with Foreign Individuals engaged in Ethnological pursuits, and with the most intelligent British residents in the various remote Settlements of the Empire."

THE GREAT COMET.

To the Editor of the Times.

TIMES, *March* 21. 1843.

SIR,—I wish to direct the attention of your astronomical readers to the fact, which I think hardly admits of a doubt, of a comet of enormous magnitude being in the course of its progress through our system, and at present not far from its perihelion. Its tail, for such I cannot doubt it to be, was conspicuously visible, both last night and the night before as a vivid luminous streak, commencing close beneath the stars kappa and lambda (κ and λ) Leporis, and thence stretching obliquely westwards and downwards between gamma and delta (γ and δ) Eridani, till lost in the vapours of the horizon. The direction of it, prolonged on a celestial globe, passes precisely through the place of the sun in the ecliptic at the present time,—a circumstance which appears conclusive as to its cometic nature.

As the portion of the tail, actually visible on Friday evening, was fully 30 degrees in length, and the head must have been beneath the horizon, which would add at least 25 degrees to the length, it is evident that if really a comet, it is one of first-rate magnitude; and if it be not one, it is some phenomenon beyond the earth's atmosphere of a nature even yet more remarkable.

I have the honour to be, Sir,

Your obedient servant,

J. F. W. Herschel.

Collingwood, *March* 19*th*.

P.S. Had there been any post last night, this communication would have been made a day earlier.

8 p.m., *March* 19.—The tail of the comet, for such it must now assuredly be, is again visible, though much obscured by haze, and holding very nearly the same position!

NEW PUBLICATIONS.

The following publications have been received:—

1. Essai sur les Glaciers et sur le terrain Erratique du Basin du Rhone, par Jean de Charpentier. One volume 8vo, pp. 363. With Maps and Plates. 1841. *From the Author. This valuable work is already well known in Britain, through the medium of this Journal and the writings of our geologists.*

2. The Year-Book of Facts in Science and Art, exhibiting the most important discoveries and improvements of the past year. 12mo pp. 283. With numerous Engravings. London, Tilt and Bogue. 1843. *From the Publisher.*

3. Travels in New Zealand; with Contributions to the Geography, Geology, Botany, and Natural History of that country; by Ernest Dieffenbach, M.D., late Naturalist to the New Zealand Company. In Two volumes 8vo. London, John Murray, Albemarle Street. 1843. *From John Murray, Esq., Albemarle Street, London. To those who wish to become acquainted with this interesting country in a statistical, commercial, and natural-historical point of view, we particularly recommend this valuable work.*

4. Explication de la Carte Geologique de la France redigée sous la direction de M. Brochant de Villiers, Inspecteur-General des Mines. Par M.M. Dufrenoy et Elie de Beaumont, Ingenieurs en chef des mines. Publié in 1841 ; par ordre de M. Teste, ministre des travaux publics, Tome Premiere. Quarto, pp. 825. With a large coloured Geological Map of France, and numerous illustrative cuts. Paris. Imprimerie Royal. 1841. *From the Authors. This first volume of a national work, which may be termed the Geognosy of France, is rich in important facts and generalizations.*

5. Geological Report on Londonderry, and parts of Tyrone and Fermanagh; by J. E. Portlock, F.R.S., &c. &c., Captain of the Royal Engineers conducting the Geological Branch of the Ordnance Survey of Ireland. One Volume 8vo. pp. 784. With a Geological Map, and numerous tinted Geological Sections. Dublin, Hodges and Smith, College Green; London, Longman, Brown, Green and Longmans. 1843. *From the Board of Ordnance.*

6. Rapport sur un Memoire de M. A. Bravais relatif aux Lignes d'Ancien niveau de la mer dans le Finmark. Commissaires, M.M. Biot, &c., Elie de Beaumont rapporteur. 2to. *From the Author.*

7. The American Journal of Science and Arts; conducted by Professor Silliman and Benjamin Silliman jun. Up to January 1843. *From the Editors.*

8. Annalen der Physik und Chemie. Herausgegeben zu Berlin, Von J. C. Poggendorf. Received up to No. I. 1843. *From the Editor.*

9. Journal of the Asiatic Society of Bengal; edited by the Secretary. *From the Editors.*

10. Bibliothèque Universelle de Genève. Received up to No. 84. 18th January 1843.

11. Interment and Disinterment; or a further Exposition of the Practices pursued in the Metropolitan places of Sepulture, and the Results as affecting the Health of the Living; by G. A. Walker, Surgeon, London. Longman and Company. 1843. *From the Author.*

12. Explanation of Gravity, or the Great Power causing Gravitation, Form, and Motion. Glasgow. *From the Author.*

13. Proceedings of the Academy of Natural Sciences of Philadelphia. *From the Academy.*

14. Physical, Chemical, and Geological Researches on the Internal Heat of the Globe; by Gustav Bischof, L.L.D., Professor of Chemistry and Technology in the University of Bonn. 8vo. pp. 315. Longman, Orme, Brown, Green and Longman, London. *This excellent volume, the standard work on the subjects enumerated on the title page, will be found equally acceptable to the geologist and natural philosopher. No geological library ought to be without it.*

15. Vollstandiges Handbuch der Mineralogiè; von August Breithaupt. Second Volume, 8vo. pp. 406. Dresden and Leipzig. 1841. *From the*

Author. An original and valuable work. We much regret the delay in publishing the remaining volumes.

16. Rapport sur les Poissons Fossiles et l'Osteologie du Genre Brochet ou Esox; par L. Agassiz. Neuchâtel 1842. 2to. *From the Author.*

17. Recit d'une Course faite aux Glaciers en Hiver; par M.M. Agassiz et E. Desor. 1842. *From the Authors.*

18. Remarques sur deux Points de la Theorie des Glaciers; par M. Elie de Beaumont. 1842. *From the Author.*

19. Description of an extinct Lacertine Reptile, Rhynchosaurus Articeps (Owen); by Richard Owen, F.R.S., G.S., &c. Hunterian Professor in the Royal College of Surgeons. 1843. *From the Author.*

20. Bulletin de la Societe Imperiale des Naturalistes de Moscow. Année, 1842. N. iii. Moscow, 1842. *From the Societe Imperiale des Naturalistes de Moscow.* 8vo.

21. Aperçu General de la Structure Geologique des Alpes; par M. Studer. Mars, 1842. *From the Author.*

22. Elements of Electro-Metallurgy; by Alfred Smee, Esq., F.R.S. No. viii., which completes the work. *From the Author.*

23. The Climate of the South of Devon, and its Influence upon Health. With a Geological Map; by Thomas Shapter, M.D. Small 8vo. John Churchill, London. *From the Author. In preparing this interesting little volume, our former pupil, Dr Shapter, has had in view to illustrate the Medical Topography of the South of Devon, in a manner similar to that in which Dr Forbes has treated of the Land's-End, and Drs Carrick and Symonds of Bristol and Clifton. The author has bestowed much pains in deducing the averages of climate from the best registers to which he had access, and in the preparation of his Tables of the Statistics of Life and Disease. The Geology of South Devon forms a useful chapter of the work.*

24. L'Institut, Journal Universel des Sciences. Paris. Received up to March 2d. 1843. *From the Editor.*

25. Bibliothèque Zoologique et Paleontologique. Folio. Neuchâtel; par L'Agassiz. *From the Author.*

26. Bulletin de la Societe Geologique de France, up to November 1842. *From the Society.*

27. Comptes Rendus des Séances de l' Académie des Sciences. Up to the end of 1842. *From the Academy.*

List of Patents for Inventions granted for Scotland from 23*d December* 1842 *to* 22*d March* 1843, *inclusive.*

1. To **Robert Wilson**, manager at the works of Messrs Nasmyths, Gaskell, & Co., Patricroft, near Manchester, in the county of Lancaster, engineer, "certain improvements in the construction of locomotive and other steam engines."—27th December 1842.

2. To **Gabriel Hippolyte Moreau** of Leicester Square, in the county of Middlesex, gentleman, "certain improvements in propelling vessels."—27th December 1842.

3. To **James Morris** of Cateaton Street, in the city of London, merchant, being a communication from abroad, "improvements in locomotive and other steam engines."—27th December 1842.

4. To **Henry Samuel Rush** of Sloane Street, in the county of Middlesex, mechanic, "improvements in apparatus for containing matches for obtaining instantaneous light."—29th December 1842.

5. To **John Rand** of Howland Street, Fitzroy Square, in the county of Middlesex, artist, "improvements in making and closing metallic collapsable vessels."—29th December 1842.

6. To **Henry Beaumont Leeson** of Greenwich, in the county of Kent, doctor of medicine, "improvements in the art of depositing and manufacturing metals and metal articles by electro-galvanic agency, and in the apparatus connected therewith."—30th December 1842.

7. To **Robert Logan** of Blackheath, in the county of Kent, Esquire, "improvements in obtaining and preparing the fibres and other products of the cocoa nut, and its husks."—9th January 1843.

8. To **Charles Hancock** of Grosvenor Place, in the county of Middlesex, artist, "certain improvements in printing cotton, silk, woollen, and other fabrics."—11th January 1843.

9. To **James Gardner** of Banbury, in the county of Oxford, ironmonger, "improvements in cutting hay, straw, and other vegetable matters for the food of animals."—11th January 1843.

10. To **John Stephen Bourlier** of Sherborn Street, Blandford Square, in the county of Middlesex, engineer, being a communication from abroad, "certain improvements in machinery used in printing calicoes, silks, paper-hangings, and other fabrics."—12th January 1843.

11. To Wilton George Turner of Gateshead, in the county of Durham, doctor in philosophy, "improvements in the manufacture of alum."—12th January 1843.

12. To William Wood of Holborn, in the county of Middlesex, carpet-manufacturer, " a new mode of weaving, carpeting, and other figured fabrics."—13th January 1843.

13. To Matthew Gregson of Toxteth Park, Liverpool, in the county of Lancaster, Esquire, being a communication from abroad, " an invention or improvement applicable to the sawing or cutting of veneers."—16th January 1843.

14. To Samuel Hall of Basford, in the county of Nottingham, civil engineer, "improvements in the combustion of fuel and smoke."—18th January 1843.

15. To Joseph Beaman of Smethwick, in the parish of Harborne, in the county of Stafford, iron-master, " an improvement in the manufacture of malleable iron."—18th January 1843.

16. To Alexander Johnston of Hillhouse, in the county of Edinburgh, Esquire, " improvements on carriages, which may also be applied to ships, boats, and other purposes where locomotion is required."—20th January 1843.

17 To John Thomas Betts of Smithfield Bars, in the city of London, gentleman, being a communication from abroad, " improvements in covering and stopping necks of bottles and other vessels."—23d January 1843.

18. To Thomas Thompson of Coventry, in the county of Warwick, weaver and machinist, " certain improvements in weaving figured fabrics." 23d January 1843.

19. To Julian Edward Disbrowe Rodgers of Upper Ebury Street, in the county of Middlesex, chemist, " certain improvements in the separation of sulphur from various mineral substances."—25th January 1843.

20. To George Benjamin Thorneycroft of Wolverhampton, iron-master, " improvements in furnaces used for the manufacture of iron and in the mode of manufacturing iron."—1st February 1843.

21. To James Boydell Junior of Oak Farm Works, near Dudley, in the county of Stafford, iron-master, "improvements in the manufacture of metals for edge-tools."—1st February 1843.

22. To James Clark, power-loom cloth manufacturer in Glasgow, "an improved mode of manufacturing certain descriptions of cloths."—2d February 1843.

23. To Taverner John Miller of Mill-Bank Street, Westminster,

oil-merchant, "improvements in apparatus for supporting a person in bed or when reclining."—13th February 1843.

24. To Samuel Kirk of Stalybridge, in the county of Lancaster, cotton-spinner, "certain improvements in machinery, or apparatus for preparing cotton and other fibrous substances for spinning."—13th February 1843.

25. To Charles Thatcher of Midsomer Norton, in the county of Somerset, brewer, and Thomas Thatcher, of Kilmersdon, in the said county, builder, "certain improvements in drags or breaks to be applied to the wheels of carriages generally."—22d February 1843.

26. To John Craig of Stanhope Street, in the county of Middlesex, gentleman, being a communication from abroad, "certain improvements in machines or apparatus for weighing."—28th February 1843.

27. To Edward Bell of the College of Civil Engineers, Putney, in the county of Surrey, Professor of Practical Mechanics, "improvements in applying heat in the manufacture of artificial fuel, which improvements are applicable to the preparation of asphalte, and for other purposes."—2d March 1843.

28. To George Bell of the city of Dublin, in that part of the United Kingdom called Ireland, merchant, "certain improved machines which facilitate the drying of malt, corn, and seeds; also the bolting, dressing, and separating of flour, meal, and all other substances requiring to be sifted."—2d March 1843.

29. To James Bullough of Blackburn, in the county of Lancaster, overlooker, "certain improvements in the construction of looms for weaving, and also in possession of certain improvements in the same which have been a communication from abroad."—4th March 1843.

30. To John Thomas Betts of Smithfield Bars, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in the manufacture of metal covers for bottles, and certain other vessels, and in the manufacture of sheet-metal for such purposes."—7th March 1843.

31. To Jules Le Jeune of North Place, Regent's Park, in the county of Middlesex, engineer, "improvements in accelerating combustion, which improvements may be applied in place of the blowing machines now in use."—7th March 1843.

32. To Thomas Howard of Hyde, in the county of Cheshire, manufacturer, "certain improvements in machinery for preparing and spinning cotton, wool, flax, silk, and similar fibrous materials."—11th March 1843.

33. To Charles Payne of South Lambeth, in the county of Surrey, chemist, "improvements in preserving vegetable matters when metallic and earthy solutions are employed."—13th March 1843.

34. To William Longmaid of Plymouth, accountant, "improvements in treating ores and other minerals, and in obtaining various products therefrom, certain parts of which improvements are applicable to the manufacture of alkali."—13th March 1843.

35. To William Barker of Manchester, in the county of Lancaster, mill-wright, "certain improvements in the construction of metallic pistons."—16th March 1843.

36. To Joseph Whitworth of Manchester, in the county of Lancaster, engineer, "certain improvements in machinery, or apparatus for cleaning oads, and which machinery is also applicable to other similar purposes." —22d March 1843.

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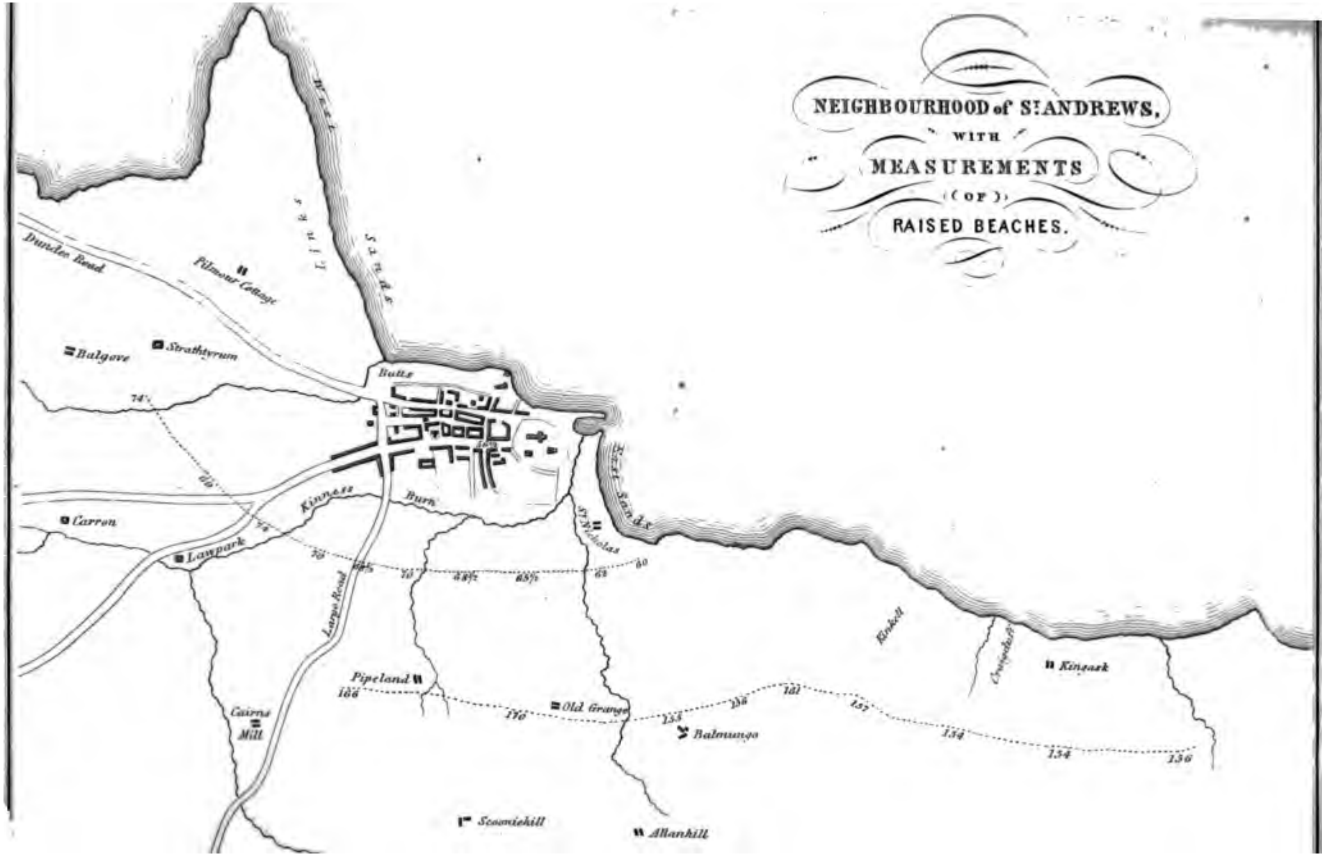

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RAISED BEACHES.

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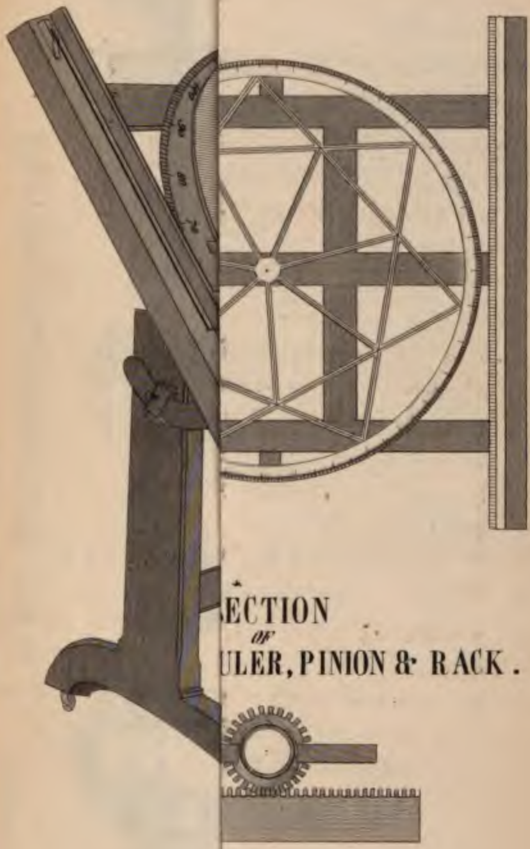
OF
& BRASS WHEEL



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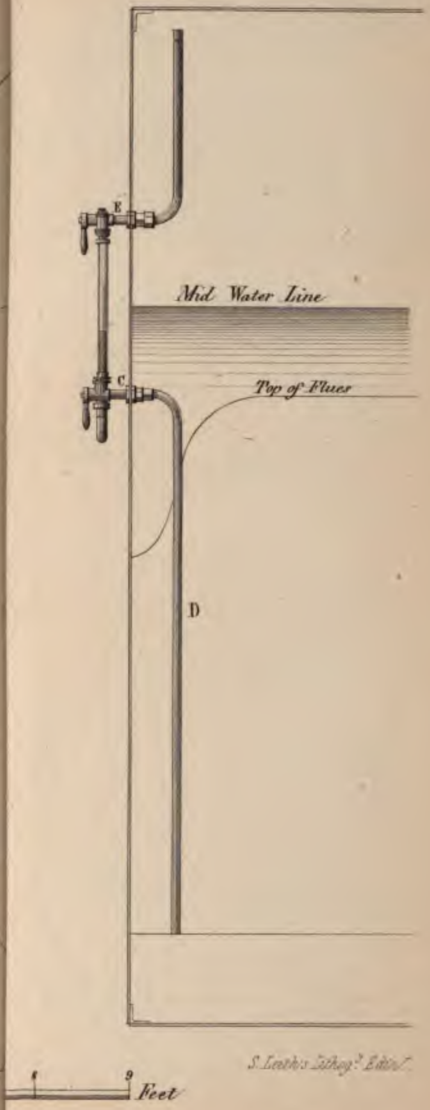

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